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NEW SERIES

VOL. 47 APRIL–JUNE, 1945 No. 2

THE RORSCHACH TECHNIQUE IN THE STUDY OF PERSONALITY AND CULTURE[1]

By A. IRVING HALLOWELL

PARADOXICAL as it may seem, anthropologists, although recognized as students of Man, have not, until recently, been much concerned with People. They have been interested in the *bones* of people and the *heads* of people; they have toiled endlessly in desert and jungle, cave and river valley to recover the artifacts of *extinct* people; they have gone into the far corners of the globe, sometimes risking life and limb to *talk* to people; they have learned and recorded the *languages* of non-literate people; they have used people as *informants* in order to discover what they could about social and economic institutions, manners and customs, string figures, art styles and systems of religious beliefs—but it is only recently that some anthropologists have begun to look their erstwhile informants straight in the eye and to think about them as *people*. Anthropologists, that is to say, have aimed to collect data that would permit them to make valid generalizations about the biological, linguistic and other cultural *attributes* of human populations considered in their group aspects, rather than about the component individuals of such populations considered as functioning personalities. This is what led Sapir to observe that: "It is what all the individuals of a society have in common which is supposed to constitute the true subject matter of cultural anthropology and sociology. If the testimony of an individual is set down as such, as often happens in our anthropological monographs, it is not because of an interest in the individual himself as a matured and single organization of ideas but in his assumed typicality for the community as a whole."[2]

On the other hand, those who have been professionally concerned with the study of human psychology—academic psychologists, psychiatrists and psychoanalysts—have not been primarily concerned either with the cultural or the psychological attributes of human groups. They have devoted themselves to observation of concrete individuals in the laboratory, in the clinic, the hospital or on the analytic couch. And since the individuals with whom they dealt had a more or less homogeneous cultural background they were not led to consider problems of group differences in personality structure, atti-

[1] Paper read before the Chicago Society for Personality Study, November 29, 1944.

[2] Sapir, 1932. (See bibliography at end of article.)

tudes, or motivation. They *were* compelled, however, to devote a great deal of attention to individual differences of all kinds as a phenomenon of *intra-group* variability, especially psychopathological deviations. Such differences were of theoretical as well as practical interest. The study of personality—how the experience of the individual is structured, how he functions as an integral whole in relation to his impulsive life and socially sanctioned goals—has emerged from a background of study in which the person, as such, commanded increasing attention.

It is interesting to note, therefore, that the interest in common problems now being manifested by anthropologists and professional students of human psychology has converged upon the study of personality. There have been developments in both groups that have promoted this.

It now appears that when cultural anthropologists were building up as accurate a picture as possible of the manner of life of the non-literate peoples of the globe they were, at the same time, accumulating data that were to prove of great value to students of human personality as well. But, being psychologically unoriented at first, the anthropologist was not aware of all the psychological implications of his data. While he often referred to the acquired nature of culture as a matter of course, and sometimes to processes of learning or conditioning, or even to culturally determined attitudes, nevertheless, he did not consider it his task to investigate or describe the "group character" or personality organization of the people whose culture he studied. It is significant, I think, that he often referred to them as "carriers" of the culture, a usage not only implying some kind of dichotomy between man and culture but entirely devoid of the faintest suggestion of any integration of culture and personality. No doubt this is partly attributable to the fact that for a long time cultural anthropologists thought and wrote about culture itself in almost exclusively analytic terms. "Culture trait" rather than "pattern" was a key word then. About the same time, too, many psychologists were talking in terms of *personality traits* rather than in integral or structural terms. Consequently it is not strange that the anthropologist failed to see that a profound psychological problem was actually intrinsic to his data. He did not ask himself how societies with all the widely different patterns of culture with which he was becoming more and more familiar could function, if there were not some intimate connection between the personality structure of their component individuals and the systems of culture they exhibited. He did not see that the very existence of a human social order is at the same time a problem in personality organization.

This problem could not be appreciated by either anthropologists or students of human psychology until a working hypothesis about the nature of human personality as a structural whole had been developed. Neither academic psychologists nor psychiatrists of a generation ago had much to offer. It is

here that psychoanalysis enters the picture. It was Freud who gave pre-eminence to the study of personality as a problem in psychic structure, who advanced hypotheses to account for its genesis and sources of variability, and elucidated the mechanisms that were intrinsic to its complex functioning. Besides this, he showed how psychic expressions of the individual—in speech, dreams, and so on—could be read as signs pointing to unconscious processes that were significant for a fuller and deeper understanding of the dynamics of his personality. Finally, Freud stressed the very earliest experiences of the child in the family as of primary significance in the moulding of personality. And here, for a moment, the anthropologists had their innings! It was clear to them that Freud was talking about a provincial type of family and methods of child rearing. They knew it was not "like that" everywhere. Since they had also read Freud's book *Totem and Taboo* in which he attempted to give a psuedo-historical rationale for his theory and, besides this, drew analogies between "primitives" and "neurotics," naturally anthropologists were skeptical.

It soon dawned upon them, however, that there were implications in Freud's theory that he himself did not envisage clearly: that parents or others who were responsible for child training were only surrogates of a larger cultural whole, and that the same socialization process that they had always assumed to be necessary for the transmission of culture from generation to generation was, at the same time, the process in which the personality of the individual was structured. Besides, anthropologists already had emancipated themselves from biologistic theories of behavioral determination—regardless of whether the fateful determinants were phrased in terms of genes, constitution or race—so that they, even more than Freud and many other psychiatrists, were ready to hypothesize some connection between the structure of personality and patterns of culture. Indeed, Freud himself adhered to a recapitulation theory, combined with the idea that acquired characteristics were inherited, to the end of his life.[3] In other words, by discarding some of the biologistic dogmas of Freud's theory, it was possible to make the same basic assumptions about the genesis of personality organization in the individual as had been made about the transmission of culture, viz., that the basic processes involved were the social interaction of individuals and adaptive learning. What varied from society to society were the specific conditions of learning, what was learned and the different roles which individuals were groomed to play.

At this point the anthropologist was faced with a new set of problems. Although he had accumulated considerable factual information on child care in different societies, from which it could be inferred that the earliest experiences of the child varied considerably from one society to another, sufficiently detailed information of crucial importance was often lacking, as well as sys-

[3] See Hallowell, 1939.

tematic data on actual processes of learning. Nor did he have very much information, often times, on the actual behavior of adults. In short, it soon became obvious that new field observations and new methods of securing relevant data were required. The behavior of infants and children was more carefully scrutinized, and the relations of parents and children and the socialization process began to attract more and more attention. Life history documents in the form of biographies also began to appear.

But even these sources of data—important as they are—leave one basic methodological problem untouched. How is it possible to make sound generalizations about the personality organization of a whole group of people without adequate samples of the personality pictures of a representative number of individuals? And how is such information to be obtained?

Even though primitive societies often are small in numbers, homogeneous in culture, and we manage to obtain a few biographies or observe with the greatest care the relations of parents and children, how can we be certain that our cases are typical, that they will bear the weight of all the generalizations we care to make about the group as a whole? Furthermore, modal characterization without an established range of variability is precarious.

Thus it soon appeared that what was badly needed, if studies of personality and culture in primitive societies were to yield reliable scientific data, was some device that would (a) provide an integral picture of the personality organization of the individual; (b) be adaptable to use among non-literate groups; (c) that would elicit data under semi-controlled conditions,[4] so that valid comparisons could be made, and (d) that the results would lend themselves to group characterization as well as provide data on intra-group variability in personality structure.

Modern techniques of the "projective" type[5] seem better adapted than others to meet such requirements, and among these the Rorschach technique in particular has achieved noteworthy eminence.[6] To my knowledge it was the first projective technique to be used among non-European peoples[7] and, in the past few years, its usage has been accelerating. The time seems ripe, therefore, for an appraisal of it in the study of personality and culture.

WHAT IS THE RORSCHACH TECHNIQUE?

The raw data for personality appraisal in the Rorschach technique are obtained from the verbal responses the subject gives to a standard series of ten representationally indefinite but symmetrically structured ink-blots, presented to him one by one in a set order. Five of the blots are entirely black,

[4] Well-controlled situations as understood in the physical and biological sciences are almost impossible to set up except in a laboratory. See Greene, 1941. [5] Frank, 1939.

[6] Rorschach, 1921. Beck, 1937 and 1944. Klopfer, 1942 (Bibliography, 370 titles). See also Hertz, 1942 (Bibliography, 317 titles). [7] Bleuler and Bleuler, 1935.

but with variegated tonal values, three are in colors, and two combine black with red. The blots used were selected with respect to certain intrinsic properties after many years of experimentation with thousands of trial blots by the inventor of the technique, Dr. Herman Rorschach, a young Swiss psychiatrist, who died in 1922. By means of an initial suggestion that he say what they might be, the subject is motivated to invest the stimulus fields successively set before him with representational values, rather than to see them as meaningless forms or empty designs. Whatever he says is written down by the administrator of the test.

Because of the non-pictorial and unconventional character of the blots, they are open to practically an unlimited variety of interpretations, especially since the cards on which they are printed may be held in any position by the subject and either the blot as a whole or any part of it may elicit his attention. The subject moreover, may take as much time as he wishes to make his answers and he may give as many or as few of them as he feels inclined. In other words, he is given complete liberty, within the limits of the situation as defined for him, to express *himself*. Whatever is seen, therefore, is bound to be a highly individualized product; the responses given must necessarily have reference to the subject. For it is *he* who projects meaning into objectively meaningless forms; it is *he* who gives few or many answers, selects the whole card or a part of it for interpretation, makes use of the color or ignores it, etc. And, in actual practice, it is found that the blots not only attain meaning in an intellectual sense; they also produce affectively toned responses through associative processes that penetrate deeply into the experience of the subject and provide clues to the nature of his emotional life. When systematically analyzed and appraised, not only in their quantitative and qualitative, but particularly in their configurational aspects, the responses of any subject are utilized as psychologically significant indicators from which his personality structure and the characteristic manner in which he functions as a human being may be inferred.

CAN RESPONSES TO THE RORSCHACH FIGURES BE SECURED FROM NON-LITERATE PEOPLE?

This is an elementary, but crucial question. One of the assumptions of those who have used the Rorschach in our society is that most adult subjects will give one or more responses to each of the ten figures. Children are granted a somewhat wider latitude than adults, but rejection of cards is always evaluated with respect to its possible diagnostic significance after six or seven years of age. It is obvious, then, that a minimum of responses must be made by the subject—otherwise no interpretation is possible. Beck, for example, considers 20 to 30 responses medium, 35 and up high, 15 and lower, low.

In 1938, when I first experimented with the Rorschach among the Saul-

teaux, I was not at all certain that it would prove practical in application, for at this date the only published material on non-European subjects was the pioneer study of twenty-nine Moroccans by the Bleulers.[8] A few other anthropologists, however, were also giving the Rorschach a trial at about the same time[9] and some of the results of these initial investigations have now been published,[10] or will be published.[11]

It turned out that 52 per cent of my adult subjects[12] gave between 21 and 40 responses (average for the series 27, range 8–129), and 47 per cent of the children produced 11–20 answers (average 18.3, range 4–61). Even the six-year-old children averaged 17.1 responses. The average number of responses of the male Alorese, 29.1, is even higher than among the Saulteaux adults.

While it is conceivable that some non-literate group might fail to produce sufficient responses to the Rorschach figures, it now seems quite unlikely. Since the pioneer investigations above mentioned were initiated, even more systematic and large-scale uses of the Rorschach technique among primitive peoples have been undertaken. Although this material and much of the earlier data remain unpublished, a conservative estimate of the actual number of

[8] Bleuler and Bleuler, 1935. In this study an underlying "racial" factor is invoked to account for the results obtained, which differed considerably from those obtained from European subjects, tested by one of the authors, a Swiss psychiatrist. Interestingly enough, certain cultural factors are also stressed, despite the obvious biologistic bias of the authors.

[9] Jules Henry (Pilagá), Cora DuBois (Alor), Dimitri Shimkin (Wind River Shoshone), Irving Goldman (Carrier), Ruth Bunzel (Chamula), and Dr. Salomon Asch had obtained twenty-seven protocols from Hopi girls.

[10] Henry, 1941. Schachtel and Henry, 1942. DuBois, 1944. The Rorschach data collected by Du Bois is analyzed by Oberholzer (Chapter 22).

[11] Shimkin's work, based on 185 subjects, ranging in age from five to eighty-four years, is still in press. On April 12, 1941 he wrote me that "the Rorschach researches formed a part of a large socio-ethnological program undertaken jointly by our Department (i.e. the Department of Anthropology, University of California) and the Institute of Child Welfare of the University, and the U. S. Indian Agency at Wind River, Wyoming . . . The active part of the program took place in 1937–1939. The analysis of the data collected has proceeded continuously and, in addition to various lesser articles, a large volume under my name 'Psychological Studies of Wind River Shoshone Children' is to be published under the auspices of the Institute of Child Welfare." For a preliminary statement of the project, see *Child Development Abstracts and Bibliography*, Vol. 13, 1939, item 1053.

[12] My Rorschach sample of the Berens River Saulteaux, a group of Ojibway-speaking hunters and fishermen living in the bush country east of Lake Winnipeg, consists of 151 protocols, heterogeneous in respect to age, sex and locale. The age range is from 6 to about 75 years, 49 of the protocols being those of children from 6 to 16 years. The proportions represented in the various age groups are comparable with the number of individuals of the same age group in the population at large. Age was substantiated in fifty per cent of the subjects. Birth dates were obtained from Mission records for all of the individuals under sixteen years. In the two major communities in which I secured my subjects, over twenty per cent of the total population is represented in my sample. The sexes, however, are not well balanced, since 63 per cent of the adults and 61 per cent of the children are males. For a preliminary discussion of my material see Hallowell, 1941.

Rorschach protocols that have been collected to date among non-literate peoples would be about 1,200. Of these, more than 1,000 are from American Indian groups, most of them obtained as part of the Indian Education Research Project sponsored by the United States Office of Indian Affairs and the Committee on Human Development of the University of Chicago.[13] In addition, a few records have been collected from Polynesian,[14] Melanesian,[15] some Munda-speaking people of India,[16] and some Sikhs of British Columbia.[17]

All these data provide a positive answer to our first question. Responses in sufficient quantities for interpretation can be obtained from non-European or non-literate peoples provided that the test is administered with care. Thus anthropology is provided with a practical psychological tool that recommends itself because (a) the equipment needed is simple; (b) there is a minimum of verbal instructions to subjects; (c) the literacy of the subjects is not involved; (d) an interpreter can be used, if necessary;[17a] (e) the subjects can take their time without affecting the results, yet relative time of responses can be used diagnostically; (f) the subjects cannot tell each other the answers, because

[13] Dr. Laura Thompson has acted as the Coordinator in this program. Dr. Dorothea Leighton and Dr. Alice Joseph (psychiatrists), and Mr. Royal B. Hassrick (anthropologist) have collected the Rorschach data. For a non-technical discussion of some of the Rorschach results, based on 105 Oraibi and 80 First Mesa children, see Laura Thompson and Alice Joseph, *The Hopi Way*, 1944, pp. 108–112. Ernestine Friedl Levy has Rorschach material on the Wisconsin Ojibwa collected while engaged in field work under a project sponsored by Columbia University and the Milwaukee Public Museum. Victor Barnouw of Columbia University also obtained some records from the Ojibwa of Wisconsin in the summer of 1944, and Leona Steinberg and Joyce Wilse obtained a series of records from the Yakima, Kwakiutl, and Coast Salish, while John J. Honigmann of Yale University has protocols he and his wife secured from Athabascan-speaking groups of Canada. Rorschach records also have been obtained from some Mexican Indian groups. Oscar Lewis and his wife recently obtained a good series, and Dr. José Gómez Robleda, in his elaborate somatic, physiological and psychological study of Tarascans made incidental use of the Rorschach technique (*Pescadores y Campesinos Tarascos*, Mexico, 1943, pp. 256–283). Unfortunately, his results cannot be equated with other studies since the colored cards could not be used because of an accident (p. 277).

[14] Cook, 1942, (Fifty males of a Mission High School, ages 16–27 years.)

[15] Fiji (Dorothy Spencer). Oberholzer (see DuBois *op. cit.*, pp. 589–590) refers to "some records of Papuans of New Guinea" which unfortunately were unscorable. However, this was due to inadequate administration of the test and not to insufficient responses to the figures.

[16] By Dorothy Spencer, whose ethnological work in India was supported by the American Council of Learned Societies and the Guggenheim Foundation.

[17] Obtained by Marian W. Smith, while engaged in field work done under the auspices of Columbia University and the Viking Fund, N. Y.

[17a] Schachtel and Henry (*op. cit.*, 1942) stress the fact that the Pilagá Rorschach protocols were collected without an interpreter. Dr. Klopfer could not discern any important differences between the Saulteaux protocols that were secured without an interpreter and those that were. In a few cases I deliberately used an interpreter despite the fact that the subject spoke some English. I felt that his responses should be recorded in the idiom in which he felt the most freedom of expression.

there are no right and wrong answers; (g) it can be used with all age groups, and (h) while certain standards in administration have to be met, these are not too rigid.

CAN THE SAME PRINCIPLES OF INTERPRETATION BE APPLIED TO THE RESPONSES OF NON-LITERATE PEOPLES AS TO THE RESPONSES OF EUROPEAN OR AMERICAN SUBJECTS?

Even granting the practicality of the Rorschach technique so far as the collection of the raw data is concerned, this question is the really crucial one. On the basis of experience derived from the use of the method in western society certain expectations are entertained with respect to the quantitative and qualitative aspects of the responses given, and also with respect to the constant and variable proportions of the average run of responses. Is it necessary, therefore, to make readjustments for each group of subjects with a different cultural background to establish a new set of norms? Or, can basic Rorschach principles be applied universally?

So far as the number and range of responses are concerned the protocols of non-literate peoples appear to approximate closely those produced by European or American subjects. *A priori* I rather expected that the Saulteaux would give fewer answers, and I wondered whether their refusal to give them—by rejecting one or more cards—would have the same diagnostic significance as among American subjects. But I discovered that out of twenty subjects who refused cards, nineteen showed other signs in their records of being poorly adjusted or maladjusted individuals. Card rejection, therefore, turned out to be an index, although not a final criterion, of psychological significance among the Saulteaux.

It is expected that the number of Form responses[18] will be numerically predominant over responses of any other category. This expectation also has been fulfilled in the protocols of primitive peoples. But the actual percentage of Form answers may be expected to vary from group to group as they do from individual to individual. Ten Pilagá children (6–12 years) gave no other responses of any kind. Oberholzer was much impressed by the fact that there was only a fractional difference between the percentage of Form answers of the Alorese and the Swiss.[19] "It remains to be seen," he says, "whether other natives, as different from the Alorese as the Alorese are different from the Europeans, exhibit the same striking fact."[20] I may say that the percentage for Saulteaux adults is about four units higher than that of these groups.

In my Saulteaux material I have been impressed by another point of close comparability with subjects from western society. Despite cultural differences, the percentage of certain content categories in my series closely approximates

[18] Answers that do not involve Movement, Color or Shading.

[19] DuBois, *op. cit.*, p. 591. [20] DuBois and Oberholzer, 1942.

the general experience of Rorschach workers. Klopfer states[21] that "at least three-quarters of the responses of most subjects deal with human beings and animals." The figure is 77 per cent for my adult series and 76 per cent for Saulteaux children. Since the Saulteaux are hunters, trappers and fishermen, I expected the percentage of answers having an animal content to be considerably higher than among Americans or Europeans. This is not the case. For adults it was 47.8 per cent, only a few points higher than the figures reported for normal and superior white adults.[22] The percentage is higher for Saulteaux children (52 per cent) but this is found to be the case among white children also.[23]

Negative reactions to the blots with bright colors, especially when the last three cards in the series are totally rejected, always has been considered psychologically significant by those who have worked with the Rorschach. One of my subjects said he thought these blots were the ugliest things he had ever seen, and he did not give a single answer to any of them. He was unique in this reaction, and there were other signs in his record as well as in his life history that indicated a psychological disturbance.

The problem of interpretation becomes singularly difficult in non-literate groups only in respect to responses that in European and American subjects are scored with strict reference to a statistically established norm. This specific problem arises in the case of Popular Answers (i.e. those responses, similar in content, that are made recurrently by subjects to the same area of various blots). In this case, however, all Rorschach experts are not themselves agreed on what is to be scored "Popular."[24] As a matter of fact, it seems to me that this problem raises no new question of principle when non-literate groups are taken into account. To ask whether the same response should be scored "Popular" among the English as the Swiss, or Germans and Americans, or the same among children of various age levels as among adults is a problem of the same order as asking whether a certain response should be scored "Popular" among the Alorese and the Saulteaux.

A compilation of actual frequencies of recurrent responses among my Saulteaux subjects revealed some interesting facts. Most impressive was the *extent* of overlapping found in the responses that had been rated "Popular" by different Rorschach experts and those that fall into this class[25] among the Saulteaux as determined by the frequency of their occurrence. All but one of the

[21] The Rorschach Technique, *op. cit.*, p. 170. [22] See Davidson and Klopfer, 1937–38.

[23] See *Ibid:* Part II. 1938. In all but five of the homogeneous age groups listed the Animal percentage is over 50 and in three groups it is more than 60 per cent. In 13 heterogeneous age groups there are only three in which the Animal percentage falls below 50, the highest figure being 58 per cent (two cases). [24] See Hertz, 1938.

[25] See Hertz, *ibid.*, pp. 18–19. The exception is animal skin, hide or pelt for Card IV, an answer that was given by 15.2 per cent of all Saulteaux subjects, whereas a response given by 16 per cent of the subjects was the lowest frequency scored "Popular" in my series.

answers that *all* scorers have rated "Popular" fall into this class, and *all* of the responses I have scored "Popular" for the Salteaux have been so scored by at least *one* Rorschach expert on the basis of experience with white subjects. The most frequently given response of all among the Saulteaux[26] is among those scored "P" by everyone.[27] Furthermore, 80 per cent of the responses I found to be "Popular" on a frequency basis are among those that Hertz determined "Popular" on the same basis, and 50 per cent are among those that Beck[28] has more recently rated "Popular" on a statistical basis. In order for "P" responses to have equivalent psychological significance in different groups it would appear that while local norms determined on a frequency basis are a necessity, the nature of the ink-blot figures is such that there is no reason to suppose that *all* the "Popular" answers of one group of subjects will be entirely different from those of every other group of subjects.

Since psychological meaning rather than statistical norms is the basis of most Rorschach interpretation, valuable insights are possible even in cases where there is a small sample of records and no group norms. Dr. Klopfer discovered this to be the case even before I had compiled any statistical data, and more recently Dr. Oberholzer made the same discovery in his experience with the Alorese material. At first he was extremely skeptical as to whether the Rorschach could be used cross-culturally. But let me quote his own words:[29]

> Despite this deficiency in the fundamentals[30] which are indispensable from a methodological standpoint for a systematic study of this kind, I undertook the task, since there existed no possibility of complying with those requirements in the case of the Alorese. There are no more than thirty-seven Rorschachs and they are not likely to be increased in number. To work out norms we need at least some hundred or more tests, perhaps more than there are Atimelangers. There is little hope that standards will ever be established.
>
> I looked around, therefore, for some other way of solving this problem. The psychological meaning and significance of Rorschach's experimental factors have proved true for Europeans and Americans. I consequently applied these principles to a few Alorese and checked the results with the ethnographer's statements concerning the psychological background of these individuals. Many hours were spent assigning rank order to subjects for personality traits. The ethnographer worked from her knowledge of the individuals; I worked blind from the Rorschach materials. The degree of coincidence between our ranking was so high that it left no doubt that the principles of the test could be applied cross-culturally.

I think we must conclude that basic principles of Rorschach interpretation can be applied to data secured from non-literate peoples. If besides a good sample of a population is obtained—at least 100—as Oberholzer says, it is possible to make use of local norms and thus achieve greater precision in interpretation.

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[26] "Animals" on Card VIII (red side figures). [27] See Hertz, 1938, p. 19.
[28] Rorschach's Test, 1944, Chapter 14.
[29] DuBois, 1944, p. 589. [30] I.e. any statistical norms.

WHAT IS THE PSYCHOLOGICAL SIGNIFICANCE OF GROUP RESULTS OBTAINED BY MEANS OF THE RORSCHACH TECHNIQUE?

Originally, the Rorschach technique was devised as a measure of individual variation in personality organization. But if an adequate sample of a population is secured the analysis of the group results should enable us to differentiate "communal," "idiosyncratic," and "role" components of personality, as stated by Kluckhohn and Mowrer.[31] This possibility already has been demonstrated in *The People of Alor*, so far as the "communal" component is concerned. For this concept closely approximates what Kardiner calls "basic personality" and DuBois "modal personality." Kardiner's inferences from non-Rorschach sources in regard to the basic personality of the Alorese received independent confirmation from Oberholzer's analysis of the Rorschach results considered as a whole. Methodologically, the assumption is that common psychological traits in any given population are due to similarities in the socialization process and common conditions of life, in short to certain aspects of culture, rather than to "race" or some other biological order of determinant. The relevance of group Rorschach results to this central problem of personality and culture in any given society is obvious. Group results obtained by the Rorschach technique can also aid us in making more valid and precise comparisons of the communal components of personality in different societies, and thus contribute to our knowledge of the nature and range of such differences within the human species.

In the days before anthropologists had demonstrated the immense range in the cultural patterns of mankind and stressed the *normality* of such variation, the manner of life of "savages" often was considered bizarre—not to say "abnormal."[31a] To the provincial minds of Europeans or Americans the familiar and the normal were confused. Now if there is a basic connection between personality and culture, we should expect to find characteristic types of personality organization occurring in some societies that, at first sight, appear just as bizarre to the contemporary student of personality as did exotic culture patterns to the early anthropologist or the layman.

Recently published Rorschach data from the Alorese provide us with such a picture. Just as analogies sometimes have been drawn between certain attitudes and practices of non-literate people and neurotics in our culture, so it is not surprising to find the same analogy presenting itself to Dr. Oberholzer when he was first confronted with the Alorese material. Yet in the end Oberholzer found the Alorese data made psychological sense in non-pathological terms. He says:[32]

I have mentioned more than once the similarity between the Rorschachs of the Alorese and the pathological findings of traumatics, some neurotics, and some schizophrenics.

[31] Kluckhohn and Mowrer, 1944.

[31a] For a penetrating discussion of psychopathological labeling see Ackerknecht, 1942.

[32] DuBois, 1944, p. 601.

The Alorese, of course, are none of these; nor are they epileptics, although their color reactions are comparable. No one of these diagnoses can hold its ground when we know everything about their tests; it is only a more or less remote analogy. The Alorese Rorschachs are something *sui generis*.

What was so phenomenal in the Alorese records was both the quantity and proportions of the various categories of color answers—and above all the characteristic use of black and white as color. Without going into detail it may be stated that the group Rorschach picture presented suggests some deep-seated disturbance or psychic trauma. While from the Rorschach data alone it is impossible to infer the precise nature of the conditions that produced the disturbance, one might suspect from what we know of the clinical significance of the use of black as color as well as the type of responses they give to the black cards that it reflected some childhood experiences, rather than experiences later in life. In our society we have psychopathic labels for the end-results of deep-seated traumas or conflicts in the individual, and we search for their etiology in his life history. The problem is the same when we are confronted with such data as we have for the Alorese. We can look for conditions that are common to the early life histories of individuals, that result from a distinctive cultural setting. The characteristic personality organization of the Alorese has its roots in poor maternal care which has resulted in a feeble development of the ego, constant frustration, fear of aggression and emotional immaturity. This is why certain analogies to psychopathic symptoms in individuals of western culture appear in the Rorschach picture of the Alorese. But the important thing to note is that the *total configuration* of the Rorschach records of Alorese individuals or the group as a whole is *not* identical with that of pathological individuals in our society. Psychic traumas have *colored* the personality organization of the Alorese but have not completely determined it. The totality of their culturally constituted experience is different so that the result, as Oberholzer phrases it, is something *sui generis*. The question, then, is not one of pigeonholing them in a "normal" *or* "abnormal" category, but of defining the conditions that have produced complex results. Careful field inquiry was required to establish these conditions.

Thus data obtained by the Rorschach technique may be used as a clue to psychological problems for inquiry, as a means of checking psychological inferences made from other data, and for making refined psychological discriminations between different groups of people as well as individuals. In the latter case the use of this technique should help us to avoid superficial psychopathological analogies between individuals in our society and the characteristic personality pictures of people in other societies which are an integral part of their cultural setting.

WHAT PROBLEMS OF INTRA-GROUP VARIABILITY IN PERSONALITY CAN BE APPROACHED BY MEANS OF THE RORSCHACH TECHNIQUE?

It was perhaps inevitable that, once the problem of personality and culture was envisaged, the question of differential "modal" personalities of different peoples should attract attention. For this concept is logically correlative with the emphasis that cultural anthropologists have given to the unique aspect of the cultural patterns of one people, as compared with those of another. Nevertheless, there are always idiosyncratic components of personality to be considered. No two individuals are psychologically identical in every respect, even though their personality structures may exhibit features shared in common with other individuals of their society. To limit our investigations to "modal" personality would be to ignore many interesting questions. Here again, the Rorschach technique is peculiarly relevant, since variability in personality structure as well as an integral picture of personality functioning can be given precise meaning in Rorschach terms. I shall only refer to a few sample problems here. There are many others.

One of the most obvious questions is the *range* of variability in personality organization that is to be found under different cultural conditions. The cultures of non-literate peoples often have been characterized as more homogeneous than the culture of western man with its vaster population base, local and national variations of major cultural patterns, its class structure, and in some places the occurrence of a caste system. Does this imply that the range in personality structure is perhaps smaller in non-literate groups? Such a question could be answered if we had representative Rorschach samples of individuals in a number of non-literate societies.

A related question concerns the relative incidence of psychoses and neuroses in societies with quite different cultural patterns.[33] Perhaps this question never can be answered satisfactorily in view of the acceleration of acculturation[34] processes in recent decades. But Rorschach surveys of various populations might be a first step, if followed up by clinical validation of the findings. In my Saulteaux sample I have discovered some signs of neuroses but there is no clear evidence of psychotic cases.

Kluckhohn and Mowrer refer to a "role component" of personality, and Linton to "status personality." My Saulteaux sample indicates some interesting differences between men and women, but I have not yet fully analyzed these. It is also possible that in Saulteaux society men who specialize in conjuring share some personality characteristics that set them off from other men. I am not at all sure that this will turn out to be the case, but my Rorschach records include about a dozen cases of such men and should throw some light

[33] Various aspects of this far-from-simple problem are discussed by Ackerknecht, *op. cit.*, especially p. 55 ff. [34] Linton, 1945, p. 130.

on this question. There are practically no data in the literature on the personality of Algonkian conjurers.[35] But, as Ackerknecht has pointed out,[36] the old tendency to view the shaman or medicine man as a kind of madman still crops out in general discussions, even in the writings of anthropologists. It would also be interesting to know whether "favored children" among the Blackfoot exhibit definable personality components that can be attributed to the role for which they are groomed.[37]

As DuBois and Kardiner have demonstrated, Rorschach data also can be used as an independent check upon the personality organization of individuals as deduced from life histories.[38]

Since I had a fair sample of adults in my Saulteaux series, I grouped my cases in terms of relative psychological adjustment or maladjustment, as shown by the Rorschach. The over-all results were as follows: well-adjusted, 10.7 per cent; adjusted 33.3 per cent; poorly adjusted 44.1 per cent; maladjusted 11.7 per cent. What significance these proportions have I cannot say at the moment, but I suspect that they may be related to acculturation processes. This hypothesis is partly borne out by the fact that among the Inland Indians, who are *least* acculturated, well-adjusted or adjusted individuals as compared with poorly or maladjusted individuals constitute 50 per cent each in my sample. Among the Lakeside Indians, on the other hand, who are *most* acculturated, poorly adjusted or maladjusted individuals constitute 60 per cent of the sample. Moreover, in this latter locality are found more *extreme* cases of maladjustment, a little more than twice as many as Inland. But there are also more cases of very good adjustment, especially among the women.[39]

While these data are suggestive, rather than conclusive, I think they indicate the possible value of the Rorschach technique in acculturation studies where we need more information on the actual processes of re-adaptation which individuals undergo,[40] before any new socio-cultural equilibrium is reached, if it occurs at all. On the Berens River it seems to me that future retesting might even show that the proportion of maladjusted individuals in the Inland group had increased while those in the Lakeside band had decreased.

The value of the Rorschach technique as a tool in the investigation of problems of personality and culture in non-literate societies seems unquestionable. It is not a substitute for other methods of approach but it nicely supplements them. It has its own characteristic virtues as well as its own limitations. But this is likewise true of other techniques, so that a combination of procedures and various angles of approach seems calculated to achieve the best results. Other projective techniques, for example the Murray Thematic

[35] See Hallowell, 1942a. [36] *Op. cit.*, pp. 36 and 50–53.
[37] Hanks, 1939. [38] DuBois, *op. cit.*
[39] For a more detailed technical discussion see Hallowell, 1942b.
[40] For an introductory discussion of this problem see Hallowell, 1945.

Apperception Test, already have been used among non-literate peoples with success.[41] The fundamental problem is to find the best available procedures for obtaining reliable data so that satisfactory answers can be obtained to certain questions. I have assumed that anthropologists will want to secure more precise answers to questions that concern personality and culture, so I have tried to appraise the present status of the Rorschach technique as one of the best available means to that end.

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[41] See W. E. Henry, 1944, who also has a manuscript on the analysis of Hopi material.

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MATERIAL CULTURE AND THE ORGANISM

By EMERSON F. GREENMAN

IN THE past seven years the developmental aspect of human material culture has been before me each field season in unmistakable clarity on the northern shore of Georgian Bay, where there is a progressive sequence in the forms of primitive implements corresponding to the antiquity of the abandoned beaches in which they are found. On the highest beach, which is 297 feet above the present level of Lake Huron, large, roughly made implements of quartzite are found, and such later forms as arrowpoints, pottery and implements of ground stone, are missing.[1] Lower down and nearer to the present shore is another of these raised beaches in which the artifacts, found in positions indicating their presence there when the lake was at that level, represent an advance over those of the uppermost beach; new types appear, and new materials like flint and slate are used.[2] But still there is no pottery, and some of the forms of the older site are present. On the lowest beach, which lies 28 feet above the present lake level, the full complement of material culture characterizing most of the later sites in northeastern North America is present. Potsherds are abundant, ground stone implements predominate, most of the projectile points and knives are made of flint, and even here on this one site there is a change in the form of the projectile points, one which is definitely correlated with the passage of time as indicated by the strata in which they are found. Finally, in an early historic cemetery not far distant, some of these implement-types of the lowermost beach occur in association with objects introduced by Europeans, and here one observes the change from the old aboriginal cultures, and a step in their gradual disappearance.

The period represented in these four sites has been estimated at some ten to fifteen thousand years.[3] The picture of technical improvement presented by them, a process beginning at a time when the edge of the last continental glacier was within one hundred miles,[4] is but part of another which tells the story of the progressive development over a much greater period of time in the things made by men.

These evidences provoke inquiry as to how such a process of technical improvement operates, and how it could have begun. It is undeniable that humans are the physical descendants of simians resembling those living today in various parts of the world. Going backward in time human material culture becomes less complex and smaller in extent. Continuing this reversed movement we are brought inescapably to a being possessing the minimum requirements of humanness, and however one may list those requirements, that being

[1] Greenman and Stanley, 1942.
[2] Greenman and Stanley, 1941. (See bibliography at end of article.)
[3] Greenman, 1943. [4] Greenman and Stanley, 1942, p. 528.

was in physical make-up and in material culture much closer to the ape than to modern man. In attempting to explain how man, "beginning" with possibly no more than a flint knife, a few objects of bodily decoration, and a language consisting of a dozen of so phrases, could develop into a manufacturer of mowing machines, woven clothing and radios, it is conventional to say that the development of a superior brain was the cause. As an example, it has been said that "The first tools were invented, so far as we can discover, in a rationalistic manner, that is, through imagination and reason."[5] The thesis to be presented in this paper is that the pattern of a technical innovation was suggested by accidents in the environment, and that the brain, in utilizing the new form, became itself improved.

In the creation of new and improved types of implements accidents of this sort, verified alike by recorded experience and in the forms of implements themselves, stand out as of primary importance in the interpretation of the genesis and evolution of material culture of the human type. They involve relationships between natural objects, between natural and artificial objects, and between two or more artifacts. For occurrences of this type I use the term "juxtaposition."[6] It is the same process as that described by Harrison as an "opportunist" reaction.[7] That author provides a hypothetical example of a juxtaposition when he suggests[8] that the first handle for a stone implement was invented in connection with the sharp-rimmed ovate flint hand-axes of the palaeolithic period. "The first step," he says, "may have been the hasty seizing of a handful of moss, or a loose piece of skin, to interpose between the hand and the flint, and so the discovery came within easy reach."

Such an occurrence has in it the following elements: Accidental juxtaposition of objects having a potential value when combined, the presence of a situation calling for the function they can perform in combination, a brain capable of realizing the potential value of their combination, and organs able to perform the mechanical acts necessary for combining them. In this case a new type of tool was formed. Harrison describes the process as "opportunism," which emphasizes the activity of the mind. I use the term *juxtaposition*, which emphasizes an event in the environment, and which I regard as of equal importance with the mental element. The essential feature of this event is its accidental nature.

The history of an actual juxtaposition is contained in the well-known sequence by which the socketed celt was produced during the Bronze Age in Europe.[9] The crucial stage in this series is the one in which the heightened side

[5] Ellwood, 1920, p. 76.

[6] The term is not entirely new. Apparently it was first used by Carmichael in an article in 1921, p. 61. He says: "In their previous progress our ancestors had merely affected new juxtapositions of materials already at hand. . . ." [7] Harrison, 1930, p. 108. [8] *Loc. cit.*, p. 112.

[9] MacCurdy, 1924, Vol. 2, p. 184; Read, 1920, p. 26.

ridges curve in toward one another.[10] It was at this point that the idea of connecting them was suggested, resulting in the "invention" of the round socket. The side ridges were, in short, accidentally juxtaposed, and the mind of the maker was sufficiently alert to follow the pattern presented by forces outside of his mind and independent of his intentions.[11]

Another evolutionary series in the Bronze Age involved a similar suggestion which culminated in the perforated spearhead.[12] The part of this development with which we are concerned begins with a spearhead, cast in one piece, with the socket type of haft. At the base of the socket are two projecting loops for securing the spearhead to the shaft by means of thongs. In successive forms these loops migrated upward along the stem to the base of the blade, and finally became incorporated into the blade and within its normal outline, at the same time losing their original function. In the final forms these holes became larger, producing a blade whose edges were formed by two wide bands of metal fastened to the central stem only near the point and at the base. Here a new form was given to an implement through an uncontrolled process by which the original loops were brought close enough to the base of the blade to suggest the idea of an open-work blade. The only improvements resulting were apparently the lightening of the blade and the saving of metal.

Thomas[13] describes the independent discovery by him of the bull-roarer in a way involving all the elements of the juxtaposition except the situation providing a need for the thing discovered, but in this case it was put to use and a new need thereby created. Here, the things brought together, i.e., juxtaposed, were not actual objects but the memory of actual objects. A similar type of juxtaposition resulted in 1866 in the "invention" of a crane of unusual strength

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[10] MacCurdy, *op. cit.*, Fig. 328, No. 4.

[11] An objection is raised to the interpretation of this series as illustrating the origin of the socket, by Harrison, 1926. His point is that the gap between MacCurdy's No. 3 and No. 4 is a large one, involving the use of a new technique, casting by the core method, and that the idea of the longitudinal socket was introduced along with the core-technique which made it possible. He says however: "In the absence of archaeological evidence to prove that introduced socketed implements, and the method of making them, were at the disposal of the identical bronze-workers who produced the first European socketed celts, I cannot pretend to have disproved the accepted theory, except to my own satisfaction."

[12] Evans, 1933, pp. 187–202; Read, 1920, p. 37, Fig. 22.

[13] Thomas, 1925. His account follows: "As a child in Ceylon at a small up-country school, I was much interested in the paper and bamboo contrivances the natives hung or fixed outside their *boutiques* in the bazaars to revolve in the wind. A Japanese bamboo paper knife, hung by a string through the tassel hole, revolved merrily, I found, until the torsion of the string became too strong for it. We were all devoted to slinging at this time: a pastime to which a native servant had introduced us. The first-mentioned interest led me to tie a string to my paper knife; the second, I feel, prompted me to whirl it round in the air. At any rate I did so and the result astonished me. I was the introducer of a toy which became very popular for a time, and which I only learned some 15 years later was the *bull roarer*."

simply because the man attempting to design a crane happened to come into the office of another who had made a section of a human bone revealing an arrangement of the cancellous tissue which was adaptable to the strengthening of the crane.[14]

In the series of experiments on the tool-using abilities of chimpanzees carried out by Kohler[15] the procedure was largely that of placing artificial juxtapositions before them and recording the results. In one of these experiments a banana was placed just outside of the cage of an ape, Sultan by name, and beyond his reach. Two hollow bamboo sticks, one of such a diameter that it could be fitted into the other to make a stick long enough to reach the banana, were placed in the cage at Sultan's disposal. After numerous attempts lasting over an hour in which one stick was pushed toward the fruit with the other, Sultan abandoned his efforts and took no further notice of the sticks, which were left in his cage as the experiment was to be resumed before long. During this period and without showing any interest in the fruit he picked up the sticks and began to play with them in a casual manner, placing them in various relationships to one another. When he found himself holding the two sticks end to end he pushed the thinner one a short distance into the other, and perceiving the potentialities of the longer stick thus formed, he used it immediately and successfully to draw the banana toward him.[16]

Three important aspects of the juxtaposition are contained in this achievement of Sultan's. First, it indicates that the principle operates at the level of simian intelligence. Second, the new implement was made by the ape entirely unintentionally; a new implement was actually constructed without the previous working-out of the pattern in the mind. Third, the synthetic nature of the whole procedure emphasizes the gradual character of any technical innovations brought about by means of the juxtaposition, whether among anthropoids or humans. The experimenter placed in the cage the materials out of which could be formed an implement suitable for the satisfaction of a simul-

[14] Thompson, 1943, pp. 976–978. The account follows: "It was shown by Hermann Meyer (and afterwards in greater detail by Julius Wolff and others) that the trabeculae, as seen in a longitudinal section of the femur, spread in beautiful curving lines from the head to the hollow shaft of the bone; and that these linear bundles are crossed by others, with so nice a regularity of arrangement that each intercrossing is as nearly as possible an orthogonal one: that is to say, the one set of fibres or cancelli cross the other everywhere at right angles. A great engineer, Professor Culmann of Zurich, to whom by the way we owe the whole modern method of graphic statics, happened (in the year 1866) to come into his colleague Meyer's dissecting-room, where the anatomist was contemplating the section of a bone. The engineer, who had been busy designing a new and powerful crane, saw in a moment that the arrangement of the bony trabeculae was nothing more nor less than a diagram of the lines of stress, or directions of tension and compression, in the loaded structure; in short, that Nature was strengthening the bone in precisely the manner and direction, in which strength was required; and he is said to have cried out, 'That's my crane!"

[15] Kohler, 1925. [16] *Op. cit.*, pp. 130–133.

taneously arranged need in the form of the fruit outside the cage. Under natural conditions it would be very rarely that two sticks would be brought together, one of which could be fitted into the other, near a banana so placed that it could be obtained only by using a longer stick than either of the two sections in the presence of a chimpanzee in the mood to play with the two sticks, and clever enough to see the relation between their fortuitously achieved combination and the otherwise unobtainable fruit.[17]

In this achievement of Sultan's a process is shown whereby a fairly complex tool was made by a being similar to the anthropoid ancestor of man, and it is a fair presumption that various types of implements were discovered by that ancestor in the same manner. There is a difference between this act of the chimpanzee and the process underlying the making of the longitudinal socket in the Bronze Age, the discovery of the bull-roarer by Thomas, and of the means of strengthening the crane as described by Thompson. In Sultan's case the tool was actually made before its significance was appreciated. In the case of the socket the pattern of the new hafting mechanism was suggested by the incidental convergence in space of the edges of the side-ridges, and from the facts in the case it seems probable that when they were sufficiently close to one another the mind completed the picture, seeing them brought together as a closed socket. In the cases of the bull roarer and the crane no objects were brought completely together in space, although in the latter if the two men occupied adjoining offices their proximity would have been a contributing factor. In both, the time element was the determining condition. Two objects with a potential value in combination were at the same time fresh in the memory of a single individual, i.e., they were "juxtaposed" in time, and here the pattern was completed in the mind, but it was not formed there. If a year had intervened between the time Thomas saw the paper knives spinning in the wind and his preoccupation with slinging it is doubtful if the bull-roarer would have been invented by him.

The juxtaposition may now be described as the creation of a new implement as the sequel to the establishment of a close spatial relationship between two or more objects, or of a close temporal relationship between the mental images of two or more objects, by natural or artificial means without foreknowledge of the result.

[17] In other experiments with Sultan and other chimpanzees where a single object, suitable in itself for use in the right way, was placed in the cage, Kohler found that "The problem is most easily solved when the distance (between goal and tool) is already virtually overcome, i.e., when the implement is already 'placed' in relation to the goal (pp. 25–26); and also that when the tool and the desired object could be seen both at the same time the correct solution followed more often than otherwise (pp. 37–38). "The best tool easily loses its situational value if it is not visible simultaneously or quasi-simultaneously with the region of the objective" (p. 59). The chimpanzees, however, could learn, and after many experiments in some cases the goal was reached when there was a wide optical distance between it and the implement.

The literature of primitive technology is full of theoretical discussions of the origins of implements through the operation of the principle of the juxtaposition, a fact which in itself constitutes a recognition of its importance. Henry Balfour[18] derives the more complex forms of stringed instruments from the musical bow which itself arose as the sound produced by twanging a bow-string without intention to produce a musical note. Balfour provided a good illustration of a juxtaposition in pointing out the possible origin of a stringed instrument with a sounding-board in the accidental resting of a shooting bow on the surface of an inverted pot or gourd. The sound of the plucked bow-string would then be intensified and the potentialities of the arrangement quickly appreciated. The same authority suggested that the cross-bow arose out of the practice of using an ordinary bow and arrow as a trap, with a release to be set off by the animal. The event in which a man setting such a trap used it as a cross-bow to shoot an animal which came unexpectedly into view is a juxtaposition since it involved the bringing together of two factors (the bow and the release) for a purpose other than that for which it was used in an emergency. The origin of the use of metals, of methods of producing fire, of agriculture and pottery, of glass and of the outrigger canoe are often assigned speculatively to a process of the juxtaposition type.[19] It is, in fact, a not uncommon experience in everyday life.

The principle of the juxtaposition, loosely defined, may be extended to the concurrence of social contexts and physical conditions through the interaction of which new types of behavior and new or improved material constructions are produced. An example of the latter is contained in a theory of the origin of mound burial[20] in which it is put forth that the desire to place the remains of the dead deeply into the earth for protection from human and animal vandals was obstructed by the "hardpan," and that as an adjustment to the situation burials in shallow graves or on the surface were covered by earth collected in the immediate vicinity from the top-soil zone. Here the relationship which (theoretically) caused the new trait is between a context (desire to insure safety of the remains of the dead) and a physical condition (the hardness of the subsoil) which made it difficult to dig with primitive tools. There is juxtaposition in this case, not between objects but between a social context and a material condition of the environment, and a new material form arose from their interaction.

In its general spirit, the principle of the juxtaposition can be applied to the emergence of language in the manner postulated by Thorndyke,[21] in which some of the incessant random vocalizations by the members of a pre-linguistic

[18] Balfour, 1924.

[19] See the following: Sayce, 1933, p. 60 *et. seq.*; *Beginnings of Pottery-Making*, Morris, 1939, pp. 20–21; Dixon, 1928, pp. 35–36; Boas, 1927, pp. 117–128; Harrison, 1930, pp. 113, 115.

[20] Greenman, 1932, pp. 286–295. [21] Thorndyke, 1943, pp. 90–97.

human group acquired meanings through their repeated connection with the experience of certain objects or acts. I would carry this hypothetical situation a little further and say that when a vocalization made in random fashion once called forth a suitable response in another individual there would be a strong motive for its repetition and stabilization as a linguistic utterance. Here again a context is necessary; a situation charged with meaning for two or more people is a necessary element in order that the same meaning become ascribed to the same sound by the speaker and hearer. Language is but a vocal gesture.[22] It is merely one physical means among several used by animals to elicit a response in another animal.[23] Speaking of his observations of the howler monkey Carpenter[24] says "Gestures are to sight what sound patterns are to hearing, and each is used to coordinate social responses over long distances." It can be said with little danger of disproof that vocalization came into use for signaling only because the mouth, lips and vocal chords were together mechanically capable of varying a physical phenomenon (sound) infinitely and speedily. If some other bodily organ could have imparted to some other physical phenomenon such variegation and speed, that means might have become the mechanism of communication. The only other human organ capable even of approaching the organs of speech in those powers are the hands. It is to the hands that the speechless individual has recourse, and it is the hands that are the instruments of the written word. If man had come to the tool-making stage before his anatomical equipment made articulate speech possible he might have developed a machine capable of varying visual, auditory or tactile phenomena fast enough to enable the communication of grammatically classified concepts.

The interplay between the making of random sounds and the context which produced reactions to those sounds is in the nature of a juxtaposition, though, like the interpretation of the rise of the practice of mound-burial, it can be more accurately described as a concurrence, since it involves the coming together of two things in time rather than in space.

New instruments have probably arisen in a very gradual manner as the result of use of those already in existence, and no doubt as the result of small variations due to changes in the materials used. An example of the former would be the production of a bowl-shaped mortar by a long-continued process of grinding on a stone. Here the utilization of the resulting bowl as a mortar, and the intentional manufacture of such an object, would not depend on the

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[22] Sweet, 1900, pp. 37–38. This author uses the term "lingual gesture."

[23] Ants use gestures: "By violent blows with her antennae, often delivered with her mandibles open, an ant urges her companion to the attack, or, on the other hand, to flight" (Forel, 1930, Vol. 1, pp. 239–240). Here the importance of the context is obvious if it is to be understood that the signal indicates that the moment has arrived for doing the thing that the communicant recognizes must eventually be done.

[24] Carpenter, 1942.

bringing together of two objects or ideas, but the chief factor of the juxtaposition, the accidental, unplanned improvement, is present, and the term can be broadened sufficiently to include such occurrences.

The creation of a new type of implement through the operation of the juxtaposition is a reaction on the part of the brain to a stimulus in the environment. It seems apparent that at the simian and proto-human levels the brain was incapable of the amount or quality of synthesis necessary to the working out of the full pattern of a new technique, even in the ape Sultan's case under compulsion of a pressing need; but that the brain was capable of "getting the idea" when the component materials of the innovation were placed together before the eyes in the right manner. The progressive evolution of technical forms had to wait upon such accidental juxtapositions.[25] Because of this dependence upon the juxtaposition it is possible to say that technical progress from the proto-human level onward was not a function of the brain alone. It is easy to believe that the brain steadily increased in power from an anthropoid ancestor by virtue of some mysterious evolutionary momentum, or through the operation of natural selection in some unspecified manner; and that this brain, having become human, began independently and by its own brilliance to conceive the rather complex processes for making the implements which developed into the successive cultures of the palaeolithic. Meanwhile the brain itself increased in power from *Pithecanthropus* to Piltdown and onward to Neanderthal and Cro-Magnon.[26] Natural selection cannot alone explain the growth of the brain[27] and that process cannot be assigned to any other cause, unless the mechanisms of its operation be shown.

The essential significance of the juxtaposition is twofold (a) as a cause of technical advance originating independently of the organism, and (b) in the effect of the technical advance in changing the habits of the organism and thereby altering its physical character.[28] In the latter I have in mind the gen-

[25] This condition is not limited to primitive times. H. S. Harrison (1936, p. 15) speaks of "the obvious mental limitations by which modern man is hampered in exploiting those of his opportunist reactions which may lead to fruitful discoveries and inventions."

[26] The Cro-Magnon brain is said to have been equal to that of modern man, but Aurignacian material culture, at the time of its appearance in Europe when it was already ancient, was negligible in comparison to that of today.

[27] "His (man's) brain might perhaps be increased in capacity—that is, by the specialization of its parts and in the number and associations of its cells and tracts—by some process of selection, based upon transmissible development arising out of the stimulating influence of the working of the brain itself. Natural selection we have discarded as ineffective or misleading, and if we are obstinate in our faith in use-inheritance, we find ourselves in difficulties" (Harrison, 1936, p. 14). "In other words, natural selection within the bounds of the single generation is an active factor in biology. . . . The really important criticisms have fallen upon Natural Selection as an evolutionary principle, and have centred round the nature of inheritable variation" (Huxley, 1942, p. 17).

[28] Hough, 1929, expressed this idea in the following: "It is easily believed that man's culture

eral effects resulting from such simple but important advances as the discovery of the use of flint flakes for cutting, the use of the club in attack and defense, the development of clothing and especially the elaboration of projectilism wherein the protective distance between the individual and the game or the enemy, already increased by the club, was effectively enlarged. There can be no doubt that such developments as these, coming very early in human history, worked great changes in the habits of man and became reflected in his physical constitution, to become permanent and cumulative through social if not through genetic inheritance.

The new or improved implement, however, had another and a more subtle effect.

I take the view that in the transition from anthropoid to human, technical and neural improvement were the result of a reciprocal relationship between the artifact and the organism, one in which each lifted the other alternately to a higher stage, and which was possible by virtue of the principle of the juxtaposition, which set before the organism something which itself was incapable of conceiving. It is because of this relationship that there is a correlation between industrial development and an increase in the power of the brain as indicated by the parallel evolution of the two in the palaeolithic period.[29] This process has its analogy in the organic realm, where the instruments for altering the materials in the environment are a part of the organism.[30]

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and his physical development went hand in hand. . . . It may be conceived that Man's advance was promoted by interactions between his consciousness and his inventions, the latter leading to changes in habit which reacted upon his physical make-up. . . ." (p. 513).

[29] This parallel development is one between material productions and the organism as a whole, although the most conspicuous anatomical changes took place in the brain and in the lower part of the face. "Evolution of man has been attended by changes in two parts of the body; the brain has expanded; the jaws have been reduced" (Keith, 1931, p. 105). Anatomical changes due to the assumption of more erect posture from the simian ancestor through Neanderthal and Rhodesian Man to Cro-Magnon could, among many other changes, be added to those cited by Keith (MacCurdy, 1924,I, pp. 375–376, 370) "The history of these structures (various portions of the brain) is closely interwoven with the differentiation of the extremities and their record reveals how that progress has advanced through many phases from ape to man until the human hand made its appearance" (Tilney, 1928, p. 836). As to the development of the forms of material productions concurrently with the increase in the power of the brain the facts are too well known to need detailed expression here. It is sufficient to call attention to the primitive nature of the artifacts associated with Piltdown (MacCurdy, *op. cit.*, Vol. 1, p. 107) whose brain "was the most primitive and most simian brain thus far recorded" (MacCurdy, *op. cit.*, Vol. 1, p. 329), as compared for example with those of Middle Mousterian type associated with the La Chapelle aux Saints skull, representing the largest-brained member of the Neanderthal species (Keith, *op. cit.*, p. 289). The discrepancies between the material productions of Piltdown and La Chapelle aux Saints appear on the other hand very insignificant when both are compared with those of the chimpanzee, which are limited to the building of crude nests, painting with wet clay, hanging of attractive objects on the body, throwing of stones, etc. (Kohler, 1925, pp. 89–100).

[30] Forel discusses the organic mechanisms of ants under the heading "Tools" (Forel, 1930, Vol. 1, p. 472).

There are many points of similarity between organic evolution and the observed course of human material culture. The latter can cogently be understood in terms of an evolutionary process which in its main outlines is identical with the same process in the organic world. Each is a series at one end of which there are structural and functional simplicity and a low degree of exploitation of the environment,[31] while at the other end there are complexity and a high degree of exploitation of the environment. The evolution of human material culture came after that of the organism and is in large measure a recapitulation of organic evolution, even in a sense a continuation of it, for the artifacts of humans duplicate the functions of the organic instruments of animals but have developed and carried those functions to levels never achieved in the animal world, yet at the same time utilizing natural forces in the same way.

Considering the whole course of organic evolution from beginning to end, the picture is one of development from organisms without implementary organs to organisms of many types with a very considerable inventory of instruments used for locomotion and alteration of the immediate environment for the purposes of acquiring sustenance and comfort. In this list are included the following mechanisms: pincer-like arrangements such as jaws and beaks and the pincers of the cray-fish; teeth, pointed for perforating and tearing, rounded for grinding and crushing; limbs for locomotion, with terminal specializations consisting of paws, hoofs, wings, fins; feet and toes equipped for gripping and preservation of balance, and with claws for prehension, grasping and tearing; tails and tentacles for prehension, and whole bodies so proportioned as to perform the same function; beaks of birds, and mandibles of insects for grasping, perforating, crushing and sawing; the horny growths of the ungulata for tearing and battering, and their hoofs for striking and battering; complex mechanisms for the injection of poison like the hollow fangs and connected poison sacks of certain snakes, and the facilities for the projection of disagreeable liquids possessed by the skunk, octopus and other animals; the electric shock device of the ray, and the stinging mechanisms of insects; the finger-like appendages of the spiders and other insects enabling the performance of rather delicate operations; and finally, the hand of the primates, combining in one instrument the grasping power of the jaws, the mobility of the prehensile tail or the elephant's trunk, and the crushing power (when clenched) of the blunt antler, over and above its tactile efficiency and its operation under the direct supervision of the most perfect visual mechanism to be found.

[31] "The distinguishing characteristics of dominant groups all fall into one or other of two types—those making for greater control over the environment, and those making for greater independence of the environment. Thus advance in these respects may provisionally be taken as the criterion of biological progress" (Huxley, 1942, p. 562). ". . . the trend of vertebrate evolution has been to free the organism from domination by its physical environment" (Hubbs, 1943, p. 109).

There is an analogy between such instruments and those made by human beings. Many in the two classes are even similar in form as well as in function; for example, awls and teeth, hypodermic syringes and the poison-ejecting mechanisms of some snakes; boat-paddles and fins; pliers and pincers. This is but one aspect of the analogy between organic and human social phenomena brought out in striking fashion by a number of writers in a recent publication: "The social group is like the single biological unit in that both are composed of units made of the same substance, similarly organized; in that both manifest the same developmental changes; in that both exhibit the same activities and the same mechanisms. The growth curve is the same for organisms and for societies. Both maintain adaptive amplification. Communication parallels transmission of impulses."[32]

The juxtaposition is a process which in its operation and in its effects bears a close similarity to the processes of variation and mutation which are together an essential feature of organic evolution.[33] It is by genetic variation, using the word in its general meaning, that peripheral mechanisms originate; that is to say, the organism, by virtue of a process entirely independent so far as is known of any activity in the central nervous system, is presented with a new bodily feature for it to use as it will.

While neuroanatomists[34] do not pretend to an exhaustive and complete knowledge of what goes on in the minds of animals on the basis of experiments in their behavior, or of the anatomical construction of the brain, some have been impressed with the correspondence between certain physical characters of the brain and the type of limb specialization from an evolutionary point of view. Tilney[35] gives a table of the forebrain indices of three classes of vertebrates each with a different type of pectoral appendage; (a) Wings, fins, paddles, (b) Paws, hoofs, claws, (c) Hands, fingers, fingernails. Those in Class (a), he says,

> do not exceed a forebrain index (both by weight and volume) of 60 percent. In the main they are all much below this figure. . . . Animals developing claws, hoofs and paws in the forelimb differentiation have a forebrain index which may rise to, but does not exceed 80 percent. For the most part it is much below this figure. . . . In that group in which manual differentiation reaches its final stages, the hand presenting fingers and fingernails, the forebrain index is always above 80 percent. It ranges throughout the

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[32] Redfield, 1942, p. 9. The quotation is from Redfield's editorial discussion of the paper by R. W. Gerard, entitled "Higher Levels of Integration" (pp. 67–87). In discussing the paper by A. E. Emerson entitled "Basic Comparison of Human and Insect Societies" (pp. 163–176), Redfield says (p. 16): "Emerson does more than state the many analogies between animal and human societies; he tells us that these resemblances constitute another instance of convergent evolution." [33] Hogben, 1940, pp. 167, 232. Payne, 1937, p. 132.

[34] "Our knowledge of the sensory physiology of animals other than man is so meager that it is difficult to picture to ourselves in what kind of a world these animals live, so far as their awareness of it goes, and in case of the more lowly forms we can only conjecture whether they have any subjective experience at all" (Herrick, 1924, p. 28). [35] Tilney, 1928, pp. 941–942.

primates from 81 percent in lemur to approximately 90 percent in man. The significance of these comparative indices takes its importance from the fact that the forebrain is the most recent accession to the central nervous system and attains its greatest expression in mammalian orders, particularly in the primates. It is the structure essential to the most highly organized behavioral regulation. Animals possessed of the greatest development in this respect manifest the greatest adaptability in meeting the conditions of their environment. They are better able to adjust themselves and enjoy a greater range of activity. The primates stand in contrast to all other vertebrates because they possess the most effective organ for the utilization of their environmental opportunities.

Tilney's work contains many more references to the correlation between the development of the central nervous system and the somatic instruments by which the organism is enabled to act upon the environment to its own advantage.[36] Once this correlation is admitted, the question which of these two elements stands in relation to the other as its cause becomes of paramount importance. A statement by Tilney represents the attitude of neuroanatomists on this point:

Many facts already exist to prove the essential relation of the lateral lobes of the cerebellum to the arms and legs. Similar indications point to expansions in the neopallium as a response to this limb specialization. This evidence seems to put beyond peradventure the question that appendicular specialization supplies the developmental motive which underlies such far-reaching differentiation in the central nervous system.[37]

I know of no claim that the somatic organs follow and result from progressive changes in the brain,[38] and the conclusions of Tilney are generally supported by others who have written on this subject. "Many authors," says Herrick[39] "have pointed out that the primate hand is the precursor of the primate cerebral cortex." G. Elliot Smith lays great emphasis on vision but in the following extract he seems to be in agreement with Tilney and Herrick.

The comparative anatomy of the brain reveals the fact that the progressive development of vision is strictly conditioned by the ability of the hands to be used as skilled instruments. . . . Vision could not have extended the range of its powers and influence in the Primates if their hands had not escaped the specialisation which in other mammals restricted their powers of adaptation.[40]

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[36] "The basic function of the cerebral cortex is the correlation and integration of the elements of behavior sequences so that these follow in ways most appropriate for promoting the welfare of the organism as a whole" (Herrick, 1926, p. 38.)

[37] Tilney, 1928, pp. 519–520.

[38] "Physical perfections of adaptations are useless, unless advantage can be taken of them by a specialized type of brain, but specialization of the cerebral architecture cannot proceed in the absence of, yet cannot create, physical specializations in evolution. . . ." Jones, 1916, p. 198.

[39] Herrick, 1926, p. 42.

[40] Smith, 1930, p. 821. Smith, 1924, p. 36, makes a statement that could be taken as a contradiction of the above: "The whole of my argument has aimed at demonstrating that it is the steady growth and specialisation of the brain that has been the fundamental factor in leading

In the article from which the foregoing is quoted the argument is that from the reptilian ancestor of the mammals onward the activities of the eyes routed more optical nerves to the neopallium, these nerves having hitherto conveyed impulses to areas of the brain concerned with unconscious activities; and that in mammals vision and movement were closely correlated and brought under conscious control.

Forty years ago MacDougall[41] unequivocally assigned to the hand of primate and human the preeminent position as the creator of intelligence. He briefly summarizes the evolution of the human hand up to the time of its complete release from other than manipulative functions as a result of the attainment of the erect posture by the human body. Henceforth there was to be little structural change in the general features of the hand beyond its denudation of hair, specialization in the direction of greater delicacy of the palmar surface and the tips of the fingers, and the achievement of greater flexibility by elongation of the digits and the relaxation of the muscles which bind the phalanges together, thus permitting greater opposability between the tips of the fingers and the palm. MacDougall thus portrays the hand as not only a grasping and holding organ but also as a highly discriminative organ of touch, capable by its flexibility and tactile refinement of accomplishing very delicate manipulative and exploratory actions. The hand is in a sense at the same time an organ for the apprehension of shapes and for the mechanical manipulation and alteration of the objects possessing those qualities; in the first feature it resembles the eye in function, but in its possession of tactile facilities another function is added to the same organ, and these together with its manipulative ability make it the supreme instrument of the organic world.

MacDougall says:[42]

> Imagine any degree of perfection one pleases in the optic qualities of the eye of an animal like the horse, one is still utterly unable to conceive of any such a world dawning upon its intelligence as that which is presented through our own senses. The mere fact of the callous hoof and clumsy limb is an impassable barrier to an understanding of the subtle and varied impression which we have supposed the eye to be capable of receiving. With a physiologically perfect organ of vision the innumerable refined modulations of form and surface of which vision gives us instant apprehension would remain, in the blank gaze of the brute, an unmeaning confusion of light and color. Without the illuminating touch of the mobile and sensitive hand the shapes of the solid objects about us would never have detached themselves from the general background of the field of

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Man's ancestors step by step upward from the lowly Insectivore status, and through every earlier phase in the evolution of Mammals—for Man's brain represents the consummation of precisely those factors which throughout the Vertebrata have brought their possessors to the crest of the wave of progress. But such advances as the assumption of the erect attitude are brought about simply because the brain has made skilled movements of the hands possible and of definite use in the struggle for existence."

[41] MacDougall, 1905, pp. 232–242. [42] *Op. cit.*, p. 240.

vision and arranged themselves in the elaborately figured relief in which, at a single glance of the eye, they are revealed to us.

The hands are sensory and manipulating organs, whereas the function of the brain is directive.[43] With respect to the causal relationship between these two types of function MacDougall says as follows:[44] "If it be justifiable at all to separate the constituents of a process so thoroughly unitary as the rise of the nervous system with its end-organs of sense and movement, one should say that the evolution of intelligence depends rather upon the elaboration of the peripheral mechanisms than upon the central process, since in the former are given the materials and conditions of development, while the function of the latter is to coordinate and transform the material thus given."

Neuroanatomists seem on the whole to be in accord with the view stated so clearly by MacDougall. That writer, and Tilney and Smith, agree that the hand gave to the human being not only a superior capacity for apprehending the environment, but that it caused the rise of intelligence, the capacity for entertaining ideas of a higher order. " . . . There is nothing in our experience," says Herrick,[45] "there are no mental powers, no skill in ratiocination or logical analysis, no capacity to forecast future events, no flights of imaginative fancy, which do not depend directly or indirectly upon sensory data."

Smith[46] is very explicit in specifying the process by which intelligence rises out of sensual perception:

> When under the guidance of vision some delicate manipulation is performed, in addition to the success or failure of the mechanical operation, and the emotions of satisfaction or disgust which the results of the attempt and the attendant circumstances excite, the action itself starts a series of impulses from the joints, muscles, tendons, and from other parts of the body, which integrate the whole process with the intimate texture of the individual's consciousness and personality. The appetites and discriminative experience and knowledge which play their part in determining whether it is worth doing and how to do it, become associated with the activities of the whole organism during the progress of the movement. It is no longer a mere matter of muscular contraction under visual control, but a complex process of integration of experience and of creating understanding and intellect.

It is evident from the citations which have now been made that neuroanatomists take the view that increase in the power of the brain has resulted from the progressive adaptations made by the terminal mechanisms. Herrick states this attitude with reference to the human brain: "Many authors have pointed out that the primate hand is the precursor of the primate cerebral cortex."[47] MacDougall is reticent about selecting any one mechanism as over-

[43] The function of the brain is "to define and facilitate the reactions called forth by sensory stimuli" (MacDougall, 1905, p. 235). "The individual human brain controls the individual's behavior" (Herrick, 1926, p. 21).

[44] *Op. cit.*, p. 235.

[45] Herrick, 1924, p. 43.

[46] Smith, 1930, p. 824.

[47] See page 223, above.

shadowing other parts of the organism in the formation of the character of the brain, but Herrick and Tilney make reassuring statements on this point.[48] If the statements by Smith, Tilney, MacDougall, Herrick and Jones are to be taken at their face value the steady increase in control over the environment by the organism throughout evolutionary history is a matter of a reciprocal relationship between the central nervous system and the peripheral mechanism. A mutation takes place in the organ, increments to the brain result from its use. This improved brain directs the use of the improved mechanism to the advantage of the whole organism throughout successive generations. A new mutation takes place in the peripheral mechanism, and it is a mutation in an organism with a better brain than that possessed by the one in which the first mutation occurred. In this process it is the brain which controls the type of environmental selection to be undergone by the peripheral mechanism, and it is the peripheral mechanism which selects variations of a genetic origin in the brain. The latter is a process of auto-selection, in which one organ determines the course of development taken by another. This reciprocal relationship between brain and implementary organ may well be the progressive factor in organic variation, bringing organic life to the proto-human level, when the artifact came to substitute the organic implement and a new reciprocal relationship of artifact and brain was introduced through the mediation of the retained organic implement, the hand.

"Mind as cause is, in fact," says Herrick[49] "the most significant of all the progressive factors in evolution." I take that statement as it stands. But the mind could do nothing without the genetic mutation, and man could not, at least in the beginning of his career, improve his artifacts without the aid of the juxtaposition. The reciprocal relationship between the brain and the

[48] "Some particular sense organ may be very highly developed—eyes in the trout, nose in the shark, taste buds in the carp—and the entire action system built up around this dominant sense. In each of these cases a different cerebral reflex center,—optic, olfactory, gustatory—is greatly enlarged, and correlation fibers from other sensory centers converge into it, so that this particular center, no matter where situated, becomes the center of highest physiological dominance, tending to control the general behavior patterns" (Herrick, 1926, pp. 34–35). "Some authorities maintain that it was the development of the brain which led to the attainments distinguishing man from other animals. Some attribute the distinction to his assumption of the erect posture. Still others ascribe all of human progress to the development of speech. Unquestionably, each of these factors is inseparably associated with human evolution. But if one among them were to be selected as the most potent developmental incentive, first choice would seem to fall most naturally to the specialized structure of the body best adapted to externalize the neural energies of the brain. Such a flexible instrument as the human hand seems preeminently fitted for these purposes. With the brain to direct its action, to expand its usefulness, the upright posture to give freer range to its execution, with speech to make its accomplishments communal, to introduce the benefits of cooperation, the hand became the master key opening all of the ways leading through the new and vast domain of human behavior" (Tilney, 1928, pp. 775–776).

[49] Herrick, 1926, p. 357.

implement, organic or extraorganic, is in this view the mechanism by which both have progressed. In the case of humans, the development of the simian hand carried the brain to the anthropoid and proto-human levels, when the appearance and elaboration of extraorganic implements carried the brain on to the level of *Homo sapiens*, long after the hand had ceased to change so far as purely mechanical features were concerned. If peripheral mechanisms have instigated and guided the development of the brain, and if "the primate hand was the precursor of the primate cerebral cortex,"[50] the object of material culture was the precursor of the human brain, the highest of the primate series.

The neuroanatomists quoted in this paper have laid emphasis upon the hand because their interest has been in the development from simian to human. In the theme of the present paper the primate hand has been employed so far as the most perfect of a long line of improving instruments, merely to illustrate the operation of the principle of reciprocal action between brain and pectoral appendage. This organ, however, was of unique importance in the emergence and improvement of human material culture. It is the hand, and not the eye, nor the power of speech, which makes the artifact. The simple manual constructions of the simian ancestor of man developed into material culture of the human type because of the mechanically universal character of the hand as contrasted to the specific nature of the analogous implements of all other organisms.

There has evidently been evolution in the material constructions of certain insects, birds and mammals[51] but the range covered in that series is very short in comparison to that of human material culture. The reason is to be found in the invariable specificity of the implementary organs of those beings other than man which make objects out of materials in the environment. The mandibles of the building insects, the beaks of birds and jaws of the beaver are limited in their manipulating capabilities, the mandibles of the insects to sawing, grasping and perforating, the pincers of the crayfish to grasping and cutting, the jaws of birds and beavers to grasping, perforating and grinding. The mechanisms of these animals enable them not only to grasp but to place objects in new positions and as a result they have become architects.

The artifacts of humans and animals differ in one very significant respect. In the making of an artifact two fundamental processes are involved, in short, putting together and taking apart. The object out of which the new construction is to be made is subjected to one or the other of these processes or, in the later stages of human material culture, to both. The earliest human artifacts of which there is any knowledge were made by taking objects apart, by removing portions of the object in such a way that what is left is so shaped as to serve the purpose in mind. In the terminology of stone-working techniques of

[50] Herrick, 1926. [51] Savory, 1928, p. 137.

the palaeolithic period this is known as the "core technique."[52] It is the core of the block of flint, formed by striking flakes from it, that was used as the implement in pre-Chellean times,[53] by a primate which in body and probably in mental capacity was closer to the ape than to any race of men living today. The artifacts of animals on the other hand are invariably made by the process of putting things together;[54] the motive and the result are architectural rather than mechanical. The difference in the two methods of artifact making is clearly a function of the two types of organic instrument used. The effective instruments of the animals and insects are of such a specific nature as to commit them to the building process. Beaks, jaws and pincers cannot be used for the reduction of an object in the making of an artifact because only one such implement, or one set of complementary implements (such as the mandibles of the ants) is possessed by a single organism, and when an object is reduced by the removal of portions of it one instrument must hold it while the other works on it if the movements of the instrument are to be accurate. It does not seem to be a lack of intelligence that keeps the ant from making an artifact by the core method. Those insects can use a very complex tool when they find it, for certain species utilize their own larvae as shuttles in the weaving of nests.[55] The larvae, from which exude thread-like filaments, are passed back and forth between leaves or twigs in the same manner that the mechanical shuttle is used by humans. The spider web is perhaps the most complex structure made by any animal below man, and its construction is possible because the spider has appendages that can be used in a finger-like fashion. Most spider webs are stationary, but one American spider constructs a web which can be pulled into a conical shape by a line to the center, held there by the spider until an insect flies into the web and is caught, and then sprung in such a manner that the strands of the web enmesh the prey and secure its capture at once.[56] Here in such differences in spider webs would seem to be the beginning of evolution in non-human types of material production, but one which failed to take the artifact any great distance from the original form because the spider, while itself resembling a human hand, is limited to the operations that a *single* hand would be able to perform if it were not attached to the arm.

Animal material cultures failed on the whole to evolve radically because no organic instruments capable of making artifacts by the core method were produced by the processes of organic evolution until the appearance of the primate hand. In contrast to the organic instruments of all other artifact-

[52] Burkitt, 1933, p. 39.

[53] Burkitt, *op. cit.*, 109; MacCurdy, 1924, pp. 104–105. Flakes were also used abundantly. See Burkitt, *op. cit.*, pp. 108–109, on the Cromerian.

[54] The ape Sultan used the opposite process successfully in trimming down the edge of a board to fit it into a tube, but this was done by biting the board (Kohler, 1925, pp. 136–137).

[55] Forel, 1930, Vol. 1, pp. 356–357. [56] McCook, 1889, Vol. 1, pp. 195–201.

making animals the effectiveness of the hand in the manipulation of objects is as the difference between a pair of pliers and a machine-lathe. In the first place there are two hands, and each is five-spatulate so that muscular and tactile influences are sprayed out as it were upon the immediate environment, contacting it at ten different points. These hands are at the ends of long movable and flexible cranes with a double span of about five feet which can place the hands without impairment of their manipulative or tactile powers in nearly every part of a sphere of that diameter. The arms are articulated with three joints, at shoulder, elbow and wrist, and the sections lying distal to each can be rotated throughout nearly all parts of a hemisphere. Finally, the greatest freedom of movement allowed by the arms is forward of the plane of the body, that is to say directly within the field of vision. Compare this perfection of adaptation to the manipulation and handling of objects with the facilities possessed by any other animal. The only grasping instrument possessed by the birds which is freed from the function of carrying the body is the beak, which is used for perforating, changing the positions of objects, and crushing. Since it is on the head its range of movement is limited by the length of the neck and its muscular arrangements. The mandibles of the ants are in the same position and are similarly limited. The crayfish has two sets of pincers bilaterally placed, and they are opposable, but their grasping and cutting function is limited. The forelimbs and hands of the beaver are well specialized and the hands are opposable for grasping, but the beaver's chief grasping organ is the jaw, which because of its control by the muscles of the neck is as limited in range of movement as the jaws of the birds. But the beavers and spiders have instruments which approach the human forelimb in freedom of movement, and it is in the works of those organisms that we see the closest approach to the material productions of humans.

The point of this discussion of the hand in relation to other organic instruments is that it reveals the emergence of a radically evolving material "culture" of animals as contingent upon the formation by mutation of an adequate organic instrument. The hand is such an instrument, and it is not by accident that it belongs to an organism with the most powerful brain, capable very early in human history of making the most of opportunities presented by juxtapositions of a complex order, and finally of creating its own juxtapositions, of "the laying of traps for suggestions."[87]

The general conclusion arrived at in this paper may be briefly summarized. Through the operation of forces directed to no end the organism is presented with the means of increasing its control over the environment. In biology the principle is that of mutation; in human material culture, juxtaposition.

[87] Harrison, 1930, p. 108. The whole passage is as follows: "It is arguable that directional research—the laying of traps for suggestions—has not yet got beyond this essential opportunism, and never will."

Through use of the new means the powers of the brain increase, and the brain in turn redirects the operations of the end-organs. By virtue of this reciprocal relationship the organism achieved the simian hand and its neural complement, an arrangement perfectly adapted to the production of extraorganic implements. The selective effect of the hand upon mutations in the brain was at the anthropoid and proto-human levels replaced by the identical effect of the extraorganic implements which, because they required operations of greater complexity and refinement, carried the brain to the human level.

This paper has been written in an attempt to interpret the meaning of the progressive element which characterizes both organic and cultural variation; specifically, the correspondence between the development of the brain and the end-mechanisms of organisms, and that between the development of the brain and the character of the extraorganic implements of human material culture. The juxtaposition is presented as the cultural analogue of the genetic mutations in those mechanisms by which organisms operate on the immediate environment. The examples which have been given show merely that this principle does exist and operate. It is inferred that improvements in material culture could not take place without it. Biological and cultural advance are held to be two aspects of the same thing, since both, according to the argument, are the result of a reciprocal relationship between implement and brain. This process has been adduced from the data of neuroanatomy, and from the interpretations of those data by neuroanatomists. Proof of its existence rests upon a logical rather than an experimental basis, for there is no way of seeing it in operation short of observations extending over a longer period than has ever been the lot of any scientific investigation. The human hand is portrayed in this paper as the *sine qua non* of human material culture, and as the organ chiefly concerned in the development of intelligence of the human type. The genesis and evolution of material culture are thus attributed to the interplay between three factors, the juxtaposition, the reciprocal relationship, and the mechanically universal character of the human hand.

This paper is the preliminary statement of a position which is largely theoretical. Its purpose is to attract the critical attention of all workers in the various fields from which its data are drawn, and to suggest lines of research in human technology which may lead to greater knowledge about the principles underlying the development and operation of human culture.

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RELATIVE CHRONOLOGY OF NORTH AMERICAN LINGUISTIC TYPES

By C. F. VOEGELIN

IN HIS *Time Perspective*, Sapir was quite certain that linguistic diversity and distribution could be converted into chronology, with such historically interesting results as demonstrating that the Eskimo-Aleut and Na-dene were the latest linguistic arrivals in North America.[1] Sapir's diversity (differentiation) was concerned with relatively great differences of morphology within a given family. Of the lexicon he was less certain; words might be borrowed on a grand scale. Boas differed from Sapir in that he argued that the morphology was open to the same influences of diffusion as the dictionary.[2] Neither Sapir nor other workers in the field could do much with non-genetic structural diversity, for linguistic structure was not (and still is generally not) stated in such a way that significant comparisons can be made between unrelated languages.

While re-proving the new map of Indian languages,[3] it occurred to me that, besides the often cited facts about linguistic distribution, we have conveniently at hand data on what may be called internal diversity. We map language (=one or more dialects), family (=related languages), and impressionistic type (=the Sapir classification: two or more linguistic families; perhaps related, as Sapir thought; perhaps acquiring features of similarity in a non-genetic way, as Boas thought).[4] Some of the impressionistic types contain few separate languages and few linguistic families. Others of the types contain many languages or families or both, and are therefore relatively diverse. Having gained a quantifiable statement of internal diversity, we can compare it with a statement based on geographic distribution for possible corroboration.[5]

The chart of internal diversity lists the numbers of languages and families mapped in terms of six Roman numerals, which are equated to the impressionistic types of the Sapir classification, as follows:

I = Eskimo-Aleut
II = Algonquian-Wakashan
III = Na-dene
IV = Uto-Aztecan-Tanoan
V = Penutian
VI = Hokan-Siouan

TYPE	LINGUISTIC FAMILIES	SEPARATE LANGUAGES
I	2	4
II	8	50
III	4	33
IV	4	19
V	16	29
VI	25	60 (±30)
Total	59	195 (±225)

[1] Sapir, 1916, p. 79. (See bibliography at end of article.)
[2] Boas, 1940, p. 219. [3] Voegelin, 1944.
[4] It should be emphasized that Sapir did not "discover" the Sapir classification; his classification is a summary of impressions which many workers gained in the field or in the library; and of a little published work of his own and others.

Converting this chart of internal diversity into chronology, on the assumption that it takes time to create diversity, the type showing most diversified development in North America,[6] and therefore representing the earliest linguistic arrival in America, is our type VI. Next earliest is type II or type V; II has twice as many languages as V, but V has twice as many families as II (we should probably weight diversity in family). Types III and IV are tied on number of families, but the scales incline to III which has more languages than IV. Type I trails as the last linguistic arrival.[7] The relative chronology of arrivals in the New World, from oldest to most recent, is VI, V, II, III, IV, I.

Turning now to the geographic spread of the six types, we meet difficulties not encountered in the review of internal diversity. It is not so easy to quantify the geographic spread. The actual distribution of type I (Eskimo and Aleut) is littoral on the whole, so that the number of square miles regularly used is less than the linear extent of the territory suggests. Type V covers the least area on the map; type IV extends over a somewhat greater area: but both types IV and V have representation in Latin America not shown on the map. The extraordinarily great areas covered by types II, III, and VI are nearly equal, one to the other, by approximate estimate. There is furthermore a theoretical objection to continuous distribution as a criterion of age. Wide continuous distribution of a language may be the result of recent spread at the expense of other languages in the area; a broken distribution generally implies more elapsed time than a smooth continuous distribution.

The geographic factor is of little use when taken in terms of size of continuous areas. But one treads on relatively safer ground in regarding discontinuous areas as convertible into chronological terms, on the assumption that a slow and intermittent process is involved when a once continuous or partially continuous area is separated by intrusions of linguistically diverse neighbors, or by migrations from a core area to an outlying area.

The latter assumption can be made for the Athabascans of our type III in the Southwest and the Oregon-California regions; the core area of type III is however quite continuous. Type III can therefore be taken as more recent than types IV and V which show a discontinuity with an implication of intruding neighbors as well as of migrations. But however discontinuous, the

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[6] Inferential history is always open to the objection of being arbitrary and therefore esthetic (a matter of choice, of whimsy); to give more than an alternative possibility it is necessary to arrange materials in such a way that one set of data corroborates another.

[6] If we knew all about all languages of the world, we could say whether or not this first linguistic arrival left relatives on other continents or islands to continue in some parallel development.

[7] Perhaps only a part of the development of type I took place in North America; for this type, there are the most persistent impressions of major developments in Asia.

distribution of types IV and V is restricted to western America.[8] Types II and VI are not so restricted; and type VI is most discontinuous. Type I is most continuous, for where breaks occur, as on the north Alaska-Canadian border, no other languages intrude.

From the point of view of both internal diversity and geographic continuity or discontinuity, type I would be the most recent arrival. Conversely, type VI would be the most ancient, that is, the earliest type to develop in the New World. Here, however, the corroboration of the two factors in our analysis stops. Other than at the extreme ends of first comers and last comers, the relative chronology of entry (from oldest to most recent) follows one order according to internal diversity of the types, another order according to the geographic continuity or discontinuity of the types. But even for those types following different orders (=the four types between the most recent type and the most ancient type), there is a partial paired correlation in our factor analysis.

CHRONOLOGY, ACCORDING TO TWO FACTORS

Diversity factor:	VI,	V,	II,	III,	IV,	I.
Discontinuity factor:	VI,	II,	V,	IV,	III,	I.

Where corroboration of the two factors fails,[9] the failure is between two types of one pair, and again between two types of another pair: the pairs as such may be opposed for relative chronology with corroboration of both factors in our analysis. Then types V and II (or in the order II and V) represent earlier linguistic arrivals than do types III and IV (or in the order IV and III).

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[8] A third set of data (ethnological), not discussed in the body of this paper, would suggest that type IV tribes were more given to migrations than type V. Intrusion of neighbors, a process assumed to be slower, is more especially implied to account for the discontinuous distribution of type V.

[9] Namely, between types V, II; and again between types III, IV. Sapir (*op. cit.*, p. 78) had no doubt that the distribution of type V tribes antedates the Algonquian part of type II distribution; but if type II were to be taken as a whole, "a comparison with the Californian Penutian group as to relative age of linguistic differentiation might well favour the former."

THE ORIGIN OF MAIZE: PRESENT STATUS OF THE PROBLEM

By P. C. MANGELSDORF *and* R. G. REEVES

IN A recent paper in this journal, Whiting,[1] concluding that anthropologists have failed to understand the botanical evidence which bears upon the origin of maize,[2] has undertaken to interpret and evaluate for their benefit the essential facts and theories involved. The results are not wholly fortunate. If there was confusion before, it is now, we fear, confounded.

It is with some reluctance that we take issue with Whiting's conclusions, for we fully recognize the right of any student to his own interpretations and evaluations. We are convinced, too, that time and space devoted to research and the publication of research are, by and large, more fruitful than time and space utilized in controversial discussions. Furthermore, we are aware that discussions on the origin of maize have on occasion in the past become acrimonious and have solved nothing. Such an outcome we earnestly desire to avoid. Nevertheless when there appears to be, as in this case, a misconception of the real nature of the problem, combined with an obvious misinterpretation of some of the facts and an omission of some of the pertinent literature, we feel that the conclusions can no longer go unchallenged.

Whiting apparently feels that the problem of the origin of maize is insoluble by botanical methods; at least he chides the anthropologists for continuing to believe that it is soluble by botanical analysis. He does not suggest how the problem is to be solved, beyond stating that "Any solution must rest upon a series of generalizations of a very high order of abstraction." With this conception of the problem we disagree perhaps more strongly than we do with his evaluation and interpretations.

It is true that the problem of the origin of maize, as of most cultivated plants, is exceedingly complex, and it is obvious that all of the evidence, botanical, archaeological, linguistic and historical, which can be brought to bear upon it should be utilized as completely as possible. But we cannot admit that the problem is insoluble by botanical methods, if we include among these methods those of genetics, cytology and physiology as well as those of comparative morphology. We venture to predict that the solution of the problem, if it is solved, will rest less upon abstract speculations and generalizations than upon new evidence slowly and systematically accumulated by acute observation and painstaking experimentation, accompanied by intelligent and testable working hypotheses which provide new avenues of attack. Such an approach

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[1] Whiting, 1944. (See bibliography at end of article.)

[2] Although *Zea Mays* is universally known as "corn" in the United States, the term "maize" or "Indian corn" is usually preferred in international usage since the word "corn" in many countries is virtually synonymous with "grain."

to the problem Whiting apparently does not understand, or he dismisses as unpromising.

To come now to more specific points of difference: Whiting's statement that the theory that corn is a domesticated form of teosinte has long since been disproved is much more dogmatic than the circumstances warrant. It is true that the teosinte hypothesis has never been completely satisfactory. With the recent evidence[3] that teosinte is a hybrid of maize and Tripsacum and hence the progeny rather than the progenitor of maize (evidence which Whiting apparently does not accept) the hypothesis has become even less satisfactory. But to say that it was "long since" disproved is straying from the facts. We did not regard it as disproved when we began our genetic and cytological investigations of maize-teosinte hybrids some fifteen years ago; many competent authorities on maize do not regard it as disproved today. Beadle[4] and Langham[5] (in papers which Whiting does not cite) have put forward new evidence in support of the teosinte theory. Weatherwax,[6] whom Whiting credits with having contributed more sound thinking to this problem than any other writer, has recently (in another paper not cited by Whiting) pointed out that several objections to the teosinte hypothesis are less formidable than they were a decade ago. In view of these facts it can scarcely be truly said that the teosinte theory has long since been disproved.

In arriving at his conclusions regarding teosinte Whiting states that the proposition that interfertility is a better criterion of relationship than comparative morphology has now been "definitely discarded as obviously invalid." It is true that interfertility as a criterion of relationship does have serious shortcomings but this can also be said about comparative morphology. Certainly it is folly to rely completely upon any single approach to the problem of natural relationships, and especially is this true when the problem is as baffling as it is in the case of maize and teosinte. Recognizing this fact we have made a serious effort to focus all pertinent evidence upon its solution. It is disturbing, to say the least, to have this evidence casually brushed aside as of no importance.

It may be appropriate at this point to take account of some of the factors which have apparently led Whiting to reject the hypothesis that teosinte is a hybrid of maize and Tripsacum. We realize that a discussion of this matter involves technical botanical details in which anthropologists must, at best, be only moderately interested. Nevertheless it would be unfair to make the accusation of misinterpretation of facts without mentioning the evidence upon which the accusation is based.

Whiting regards as an inconsistency in the hypothesis of Mangelsdorf and

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[3] Mangelsdorf and Reeves, 1939.
[4] Beadle, 1939. [5] Langham, 1940. [6] Weatherwax, 1942.

Reeves the conclusions (a) that teosinte differs from maize primarily by four segments of chromatin and (b) that the chromosome knobs of teosinte and maize are ultimately derived from Tripsacum. The inconsistency exists only if our statements are not considered in full, for we have repeatedly stated[7] that other smaller segments, less readily detected by genetic techniques, are probably also involved. More recent experiments indicate that this is indeed the case. In some of the more Tripsacoid forms of teosinte, the so-called "pure" teosintes, these smaller segments may, in the aggregate, account for a considerable proportion of the genetic differences between teosinte and maize. This evidence also agrees with the fact that the more Tripsacoid forms of teosinte have more numerous chromosome knobs than the maize-like forms. The precise mechanism by which knobs are transferred first from Tripsacum to teosinte, subsequently from teosinte to maize, is admittedly still obscure. There is no doubt, however, of the strong, though by no means complete, correlation between knob number and various external morphological characteristics which some varieties of maize have in common with Tripsacum. Hence the suggestion that chromosome knobs of maize have been derived originally from Tripsacum has already proved to be a useful one, even if it should later, on the basis of additional evidence, become modified or be discarded. At present however, it is not inconsistent either with the facts or with our other statements.

Another apparent inconsistency to which Whiting calls attention is our conclusion that the cob of maize is toughened by Tripsacum influence in spite of the fact that Tripsacum is described by botanists as having a fragile rachis. This statement, perhaps more than any other in his paper, reveals the pitfalls which confront a writer who prefers abstractions to prosaic facts. It is true that Tripsacum has a fragile rachis in the technical botanical sense that the rachis segments disarticulate readily. But the rachis segments themselves, as well as the glumes and other structures are extremely tough or horny. In hybrids of maize and Tripsacum (as well as in maize-teosinte hybrids) many of the segregates exhibit the solid or non-fragile rachis of maize plus some of the horniness of rachis tissue characteristic of Tripsacum. Such a situation is not unusual in hybrids; indeed it is to be expected.

A third inconsistency which Whiting mentions is the statement of Mangelsdorf and Cameron[8] that a high knob number is associated with (among other things) long ears, even rows of grains and strongly flattened kernels; yet the maize of the northern Plains Indians which exhibits these characters has low chromosome knob numbers. Whiting's discussion on this point is somewhat confused by the fact that he has inadvertently used the word *low* instead

[7] Mangelsdorf and Reeves, 1939, pp. 203 and 306.
[8] Mangelsdorf and Cameron, 1942.

of *high* in citing the statement of Mangelsdorf and Cameron, but the nature of the inconsistency to which he refers is reasonably clear. The answer to his contention is that although the maize of the northern Plains Indians does resemble the many-knobbed varieties of Guatemala in having straight rows and flattened kernels it differs from them quite decidedly in many other respects, in which one of the most important is the type of endosperm. The Plains varieties are predominantly flint and flour corns. The many-knobbed maize varieties of Guatemala are predominantly dent. The differences between dent corns and flint-flour corns are genetically complex. If the straight rows and flattened kernels of the Plains maize are the product of Tripsacum admixture it certainly represents much less admixture than the Guatemalan dents have received. We have shown by actual experiments[9] that segments of Tripsacum chromatin lacking knobs and almost too small to be detected cytologically can be transferred from Tripsacum to maize to produce visible effects upon the external morphology of the plant.

Whiting speaks of the "elaborate series of hypotheses" presented by Mangelsdorf and Reeves. We have regarded our hypothesis as a single tripartite hypothesis, the three parts of which, although to some extent independent of each other, combine to present an integrated and plausible picture of the origin of maize.[10] If the hypothesis, or series of hypotheses, is elaborate it is only because the problem is, as Whiting has himself emphasized, extremely complex. Complex problems, whether mathematical or botanical, seldom have simple solutions. Actually we shall probably find, when all the facts are in, that our hypothesis, or any contemporary hypothesis, greatly over-simplifies the true situation. In any case the important consideration is not whether the hypothesis is elaborate but whether it accounts for the available facts, is testable, and is useful in providing new points of attack.

More seriously in error, however, is Whiting's statement that our hypotheses rest upon the "basic assumption" that a wild corn without knobs on its chromosomes was native to the lowlands of Paraguay and Bolivia. If there is a "basic" assumption which underlies our hypotheses it is, as we have attempted to show, and as others have had no difficulty in recognizing,[11] that teosinte is a hybrid of maize and Tripsacum. This conclusion obviously tells us nothing about the origin of maize but, if accepted, it clears the way for a more critical evaluation of the remaining evidence. Neither botanists nor anthropologists have been willing, until recently at least, to ignore the importance of teosinte as a putative maize ancestor. If, however, on the basis of recent evidence we can remove teosinte from consideration except as a source of admixture with

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[9] Mangelsdorf and Reeves, 1939, pp. 160–170.
[10] Cf. Mangelsdorf and Cameron, 1942, p. 217.
[11] Cf. Weatherwax, 1942, p. 16.

maize the remaining evidence points strongly to South America as the center of origin of maize.

To the anthropologist the details of these hypotheses and the steps by which they were derived are perhaps of less interest than the concepts to which they lead. The hypotheses of Harshberger, Collins, Kempton, Weatherwax and Longley have all visualized maize as arising in Mexico or Central America, although several of these writers have admitted the possibility of a secondary center of origin in South America. The conclusions of Mangelsdorf and Reeves, in sharp contrast, point strongly to South America as the primary center of origin. Which of these conflicting concepts is more nearly correct only future evidence can finally decide.

Whiting concludes his evaluation of facts and theories with a table of probabilities purporting to show the "betting odds" that wild maize, if it is ever discovered, will be found in any one of ten possible areas which he considers. Perhaps this table possesses some merit as an original, simple and easily understandable method of presenting the writer's conclusions to his readers. Perhaps it also reflects Whiting's apparent conception of the problem of the origin of maize as a glorified guessing game. In either case we do not agree with the probabilities which he presents. But since it is a mathematical axiom that a series of guesses is still no better than a single guess, there is little to be gained by revising the table. Perhaps the greatest value of the table lies in its implications; it inevitably suggests when one carries the analogy a bit further that those who bet on races, except as an amusement, should know their horses.

Since we have repeatedly emphasized in this paper the importance of working hypotheses which are capable of being tested perhaps we may utilize this opportunity to make a brief and preliminary report upon some of the tests to which our hypotheses have been subjected and the new evidence (for the most part still unpublished) which has accrued from these tests.

To determine whether wild corn still exists in the lowlands of South America, Dr. Hugh C. Cutler, an experienced botanical explorer has, at our instigation, travelled extensively in Brazil, Paraguay and Bolivia since 1941 in search of wild or primitive maize. He has *not* discovered wild maize. He has, however, collected in southwestern Brazil, Paraguay and eastern Bolivia a group of varieties with unusual and primitive characteristics. One of these, a variety obtained from the Guarany Indians of Paraguay, but one which is rather widely distributed in the Matto Grosso and other parts of the lowlands, is the most extraordinary type of maize which we have ever studied. It possesses almost all of the known dominant genes of maize, including several which had previously been encountered only rarely. It has a low chromosome knob number and the few knobs which it possesses are small. The rachis or cob is slender and flexible; the spikelets are easily removed from it intact. The ears, in some

environments, exhibit a partially indeterminate habit of growth. Once the ear grows beyond the end of the shucks, as it frequently does, it may elongate far beyond its normal length to produce a lax spike in which the true structure of the maize ear is revealed more clearly than has ever been done by anatomical studies. Cutler's collections also show that pod corn which previously has been reported as being unknown in Peru is sufficiently common in Bolivia to be recognized by the natives who call it *paca sara* or "hidden maize." His collections also show extraordinary diversity on the eastern slopes of the Andes in Bolivia from Sacaba and Cliza on the south to Chulumani and Coroico on the north. Two large tributaries of the Amazon, the Rio Beni and the Rio Mamoré, have their headwaters in this region, and it now appears that the basins of these rivers and the land which lies between may be a promising region in which to search for wild maize. Anthropologists familiar with South America will recognize this region as one of the most difficult in America to explore. Nevertheless Dr. Cutler plans to resume his explorations when he is released from the rubber work in which he is engaged for the duration of the war. In the meantime, however, the hypothesis that maize once had its wild habitat in the lowlands of South America has received considerable indirect support, and the hypothesis has been very fruitful in uncovering new and previously unsuspected types of maize.

The hypothesis that teosinte is a hybrid of maize and Tripsacum has been further tested by transferring the principal segments of chromatin, which differentiate the two species, by repeated backcrossing, from teosinte to a uniform inbred strain of maize so that the effects of each segment may be studied separately and the segments precisely located upon the chromosomes. This work is still far from completion; genetic studies of this nature require many years to consummate. But it has already become increasingly clear, almost certain in fact, that teosinte is an anomalous species. It is apparently nothing more than maize with segments of chromatin of another species, presumably Tripsacum, incorporated into its germ plasm.[12] It is, in fact, so strongly maize-like in its genic and chromosomal composition, and it intergrades so strongly with maize that Reeves and Mangelsdorf[13] have proposed that it be no longer classified as a separate genus, Euchlaena, but as two species of Zea. More recent data indicate that this change in classification is scarcely drastic enough; genetically, teosinte is really little more than a variety of maize.

[12] The question has frequently been asked: If teosinte is a hybrid of maize and Tripsacum why should it not be possible to synthesize it by crossing maize and Tripsacum? The answer is that in species hybrids, as in games of chance, the odds against the fortuitous repetition of a particular very complex combination are almost overwhelming. Among the segregates of our hybrids of maize and teosinte we have found a number which approached teosinte in several but not in all characteristics. Perhaps a still closer approach to teosinte might be made by crossing Guatemalan species of Tripsacum with non-Tripsacoid Guatemalan maize. Unfortunately both are difficult to grow in our latitude.

[13] Reeves and Mangelsdorf, 1942.

Unpublished work of Reeves and Mangelsdorf also shows that in 54 of 55 morphological characters which were studied in maize, teosinte, and Tripsacum, teosinte either resembles one of the other plants or is intermediate between them. This study indicates that teosinte has but few morphological characters that it could not have inherited from one of its putative parents, maize and Tripsacum, and there is reason to suppose that these few characters are the result of recent mutations. In fact, the one exceptional character mentioned above can be explained even without the assumption of recent mutations. Such a situation is difficult to interpret without recourse to the hypothesis that teosinte originated as a hybrid between maize and Tripsacum, although it does not necessarily invalidate other hypotheses.

The third hypothesis that new types of maize originating directly or indirectly from the hybridization of maize and Tripsacum and exhibiting admixture with Tripsacum comprise the majority of Central and North American varieties has received considerable additional support from the studies, already referred to, of Mangelsdorf and Cameron on the maize of western Guatemala. These studies showed that a small area in the Department of Huehuetenango, an area in which Tripsacum grows in profusion and teosinte occurs as the dominant species on thousands of acres, embraces a greater diversity of maize than occurs in the entire United States. Here are found not only the most Tripsacoid varieties in this hemisphere but also remnants of the "pure" maize presumably once involved in the hybridization with Tripsacum. Here is the probable center of diversity of the Tripsacum-contaminated varieties, and in this area, significantly, chromosome knob-number is strongly correlated with the morphology of the plant. Reeves[14] in another study has shown that Tripsacoid varieties are the predominating type also in Mexico and in countries of northern and eastern South America: Colombia, Venezuela, Dutch Guiana, Brazil and Uruguay. Mangelsdorf and Cameron (unpublished) find that this is true also of Panama, Nicaragua, Costa Rica and Cuba. Maize varieties of Paraguay like those of Peru, Bolivia and Ecuador have low knob numbers.

Reeves[14] has also demonstrated a statistically significant relationship between knob number and distance from Central America. This agrees with the conclusion that maize was distributed over parts of North Central and South America before the advent of teosinte in Central America. The relationship between knob numbers and distance from Central America becomes even stronger when the Andean maize varieties are omitted from the computations on the assumption that maize was first highly domesticated in that region and became so well established there that encroachment of Tripsacum-contaminated varieties from the North has been a negligible factor.

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[14] Reeves, 1944.

Supplementing these studies Mangelsdorf (unpublished) has investigated the interchange of genes between maize and teosinte in the Valley of Mexico where teosinte occurs as a weed in the maize fields. It has been found that the incidence of hybridization is high and that (except for certain regions of the chromosomes) there is a free movement of genes between maize and teosinte. There is, in other words, no doubt that maize is being modified by teosinte admixture today; there is every reason to suspect that this process has been going on for centuries.

Thus each part of the tripartite hypothesis on the origin of maize has received substantial new support since it was first proposed. This is not to say that the problem of the origin of maize has been solved; indeed problems of this nature are seldom susceptible to a *complete* and *final* solution. The most that can be expected is that certain important facts will become established beyond a reasonable doubt. Nor is it to say that these hypotheses will not be modified as new evidence appears. As a possible example we may mention the fact that Anderson[15] on the basis of a close resemblance between a variety of maize collected from a locality on the Rio Loa in northern Chile and a Chinese variety has revived the suggestion that maize may have been introduced into America from the Orient. The fact that there are several relatives of maize in the Orient, that pop corn is used as a food in parts of the East Indies and that the cultivated cottons of America are unquestionably hybrids of Asiatic cottons and wild American species, lends some degree of botanical plausibility to the hypothesis. There is, to be sure, much evidence against it; nevertheless it merits further investigation. But should such an hypothesis prove to be valid it would probably still leave South America as the primary American center of maize diversification. The search for wild maize in South America would be abandoned but the quest for primitive types would continue unabated.

To the extent that the origin of maize has a bearing upon the origin of prehistoric cultures in America the present available evidence, in our opinion, now points more strongly than it has ever before to western South America as the region in which American maize agriculture, and American cultures based upon maize agriculture, had their beginnings.

NOTE: Since the above was written two additional papers bearing upon the subject have come to hand. Dr. Cutler's observations on pod corn in Bolivia have been published[16] with an interesting suggestion as to how this relict character may have been maintained after selection began to operate against it. And Dr. Cutler has furnished us with a translation of a paper published in Portuguese by Dr. F. G. Brieger who is engaged in genetic research on maize and its relatives in Brazil. Dr. Brieger's studies[17] tend to support one hypothesis that maize originated in the lowlands of South America. He does not, however,

[15] Anderson, 1944. [16] Cutler, 1944. [17] Brieger, 1943.

agree with us that teosinte differs from maize by only a few chromatin segments.

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THE OCCURRENCE AND WIDER IMPLICATIONS OF A "GHOST CULT" ON THE COLUMBIA RIVER SUGGESTED BY CARVINGS IN WOOD, BONE AND STONE

By WM. DUNCAN STRONG

SPEAKING of a Chinookan Indian village of eight houses on the lower Columbia River between The Dalles and the mouth of the Klickitat River in what is now the State of Washington, Lewis and Clark (in 1805) mention a plank house where "There were figures of men, birds, and different animals, which were cut and painted on the boards which form the sides of the room; though the workmanship of these uncouth figures was very rough, they were as highly esteemed by the Indians as the finest frescoes of more civilized people."[1] Here they also briefly describe the large and graceful dugout canoes of the lower Columbia having raised bow and, occasionally, stern posts decorated with carved men, bears, and other animals, the large plank houses, and the neat wooden and grass utensils used for stone boiling. Also of special interest are the sacred medicine bags which were suspended in the houses of important men. One of these contained the forefingers of fourteen enemy "Snake" Indians, whose raids prior to this time seem to have largely discouraged Chinookan settlement on the south, or Oregon shore, at and above this section of the river. This same Chinookan chief had several articles of white trade goods, and "on one side of the room were two wide split boards placed together, so as to make space for a rude figure of a man cut and painted on them. On pointing to this and asking him what it meant, he said something, of which all we understood was "good," and then stepped to the image and brought out his bow and quiver, which, with some other warlike instruments, were kept behind it."

A few days later, just below the Cascades of the Columbia, Captain Clark notes:

> About half a mile below this house, in a very thick part of the woods, is an ancient burial place. It consists of eight vaults made of pine or cedar boards closely connected, about eight feet square and six in height; the top covered with wide boards sloping a little, so as to convey off the rain. The direction of all of these vaults is east and west, the door being on the eastern side, partially stopped with wide boards decorated with rude pictures of men and other animals. On entering we found in some of them four dead bodies, carefully wrapped in skins, tied with cords of grass and bark, lying on a mat, in a direction east and west. The other vaults contained only bones, which were in some of them piled to the height of four feet. On the tops of the vaults, and on poles attached to them, hung brass kettles and frying-pans with holes in their bottoms, baskets, bowls, sea-shells, skins, pieces of cloth, hair, bags of trinkets and small bones —the offerings of friendship or affection, which have been saved by a pious veneration from the ferocity of war, or the more dangerous temptations of individual gain. The

[1] Coues, 1893, Vol. II. References to this part of the expedition are to be found on pp. 666, 673, 674, 694, 697, and in Vol. III. (See bibliography at end of article.)

whole of the walls, as well as the door, were decorated with strange figures cut and painted on them; and besides these were several wooden images of men, some so old and decayed as to have almost lost their shape, which were all placed against the sides of the vaults. These images, as well as those in the houses we have lately seen, do not appear to be at all the objects of adoration; in this place they were most probably intended as resemblances of those whose decease they indicate; when we observe them in houses, they occupy the most conspicuous part, but are treated more like ornaments than objects of worship. Near the vaults which are standing are the remains of others on the ground completely rotted and covered with moss; and as they are formed of the most durable pine and cedar timber, there is every appearance that for a very long series of years this retired spot has been the depository for the Indians near this place.

In 1814, Alexander Henry[2] describes similar carvings at a Chinookan village on the south shore below the Cascades.

These houses are sunk about three feet under ground; the entrance is at one end by a narrow oval door, with a knotty log on the inside for a ladder. Next the door and facing it is a plank partition, behind which is the fireplace, about 10 feet square, sunk two feet below the ground floor of the house. At the other end is another partition of planks, behind which a range of beds, raised three feet from the ground, occupies the furthermost end of the house and runs round on both sides to opposite the partition near the door. The front planks of the beds are carved and painted in various styles. At the end of each range are some broad upright planks, on which figures are rudely carved, somewhat resembling fluted pillars. At the foot of the chief's bed are planted in the ground at equal distances four figures of human heads, about two feet high, adorned with a kind of crown, and rudely carved and painted. Beside these figures are erected in the ground two large, flat, painted stones. On the side of each partition, facing the fireplace, are carved and painted on the planks uncouth figures of eagles, tortoises, and other animals, some of them four feet long. The colors used are white, red, black, and green; the sculpture, in some instances, is not bad.

Unfortunately, such interesting products of the woodcarver's art on the lower Columbia have almost entirely disappeared. Even the most durable woods, among which the local cedar holds a high place, cannot normally long survive the humid conditions that prevail in the Columbia region west of the Cascade Mountains. However, a fortunate accident has preserved to the present at least two of the anthropomorphic images of the type described by the early explorers (pl. 1). These specimens were discovered in 1904 by the late Henry J. Biddle while surveying a trail around the very inaccessible eastern, or river, front of Beacon Rock (sometimes called Castle Rock) on the Washington shore of the Columbia some little distance below the Cascades. The two carved wooden effigies had been inserted in a talus slope close under an overhanging crag and were thus largely sheltered from the elements. The bottoms

[2] Coues, 1897, Vol. II. References to observations by Thompson are to be found on pp. 804, 805, 836.

of the posts, though considerably decayed, were still six to eight inches deep in the rocks and stood erect, though leaning backward against the overhanging wall. Being made of cedar the effigies may well have a considerable antiquity. It seems possible that they had remained undisturbed since the time they were placed in the hidden rock shelter by the natives. Thinking that they might mark a grave Mr. Biddle, a man deeply interested in all that concerned the Columbia River, removed the rocks in the vicinity to a depth of several feet but encountered no further human evidence.[3] Stylistically, the effigies (pl. 1) speak for themselves. They are anthropomorphic, the headdresses or hats and the clearly delineated ribs being their most outstanding characteristics. Probably the effigies were originally painted but, if so, no trace of color remains and their surfaces are generally rough and decayed. The bottoms of the posts appear to have been pointed but decay makes this uncertain.

I first saw these specimens in 1924 at Mr. Biddle's home near Vancouver, Washington, and urged him to bring them to scientific notice. He had the accompanying photographs (pl. 1) made for me at that time but, to the best of my knowledge, they have since received little scientific attention. Because they represent rare survivals of an almost vanished art style on the Columbia River they seem well worthy of publication. Fortunately, documented examples of this same style were recently rescued from long obscurity by the late D. I. Bushnell, Jr. when he published sketches and illustrations of artifacts secured by the linguist-ethnologist, George Gibbs, in 1850 or 1851, from the Lower Chinook near the mouth of the Columbia.[4] In addition to a sketch of a burial canoe with an elaborate and grotesque animal prow, Gibbs collected, among other things, a bone knife handle and a wooden spoon each of which is surmounted by a small animal having accentuated, carved ribs. An exquisite, small wooden bowl with open-work "comb" ends accompanies these. The American Museum of Natural History collections from the Tlingit in southeastern Alaska contain almost identical specimens characterized by both human and animal figures with carved ribs.

This motif also occurs in the collections from other tribes of the northwest coast, but the closest known parallels to the Beacon Rock and other Columbia River effigies from the Dalles region are the Salish wooden figures with accentuated ribs, collected from the Quinault by Harlan I. Smith in 1889, examples

[3] Among the Puyallup-Nisqually of Puget Sound, who shared a very similar culture with the Chinook, objects endowed with supernatural power (which included carved wooden human heads and torsos about three feet long when stuck in the ground) were never placed with the owner's body but were either passed on before death, or carried away by a person not related to the family and buried or hidden away. This person never told where the effigies had been placed. It is also significant that among these people, and probably many of their neighbors, there is both a conceptual and linguistic identity between the human ghost and the skeleton. This has a direct bearing on ethnological-archeological correlations suggested in the present paper. (Smith, 1940, pp. 114–116.)

[4] Bushnell, 1938, pl. 5 and figs. 1, 2.

PLATE 1. Carved wooden effigies, with ribs delineated, found in rock shelter at Beacon Rock on the Columbia River, Washington. (Bottom of posts not shown. Left-hand figure top to bottom of body 22½ inches, width below mouth 7½ inches, total length 31½ inches.)

of which are also in the American Museum of Natural History.[5] In referring to this type of Salish wood carving, the authors cited state that the art style "appears to belong to an older, more rudimentary school of sculpture" than that of the better-known northern Northwest Coast style. Finally, there are a number of wooden, as well as stone, effigies and carved artifacts mostly from the vicinity of The Dalles on the Columbia River, executed in the same or in more elaborate style and often marked by ribs. These are reported to be from the Wasco or Wishram Indians, or from burial sites, such as Memaloose Island, in their vicinity. Examples of these occur in the Chicago Natural History Museum, the American Museum of Natural History, the Museum of the American Indian, Heye Foundation, and probably in other museums and collections in this country and in Canada. A thorough study of such materials would have great value but is far beyond the purposes of the present article. Such specimens are mentioned here primarily to demonstrate that anthropomorphic and animalistic effigies which suggest the dead by clearly accentuated ribs occur in perishable wood and, in some cases, are of known ethnic provenience. These facts objectively link this art style in time with that of the wooden carvings on the lower Columbia so briefly and tantalizingly described by the explorers and traders of the early 19th century.

The early white settlers on Sauvies Island, at the confluence of the Willamette and Columbia Rivers, also found Indian depositories of the dead along the eastern shore which were, in some cases, apparently associated with quite elaborately carved stone figures. Discussion with a few of the surviving white pioneers, in 1924, indicated that the burials were for the most part in trees with the stone images sometimes occurring on the ground beneath. Other buried stone sculptures that had been washed out from the banks of the river during periods of unusually high water were found here, at Wapato Island a few miles down the Columbia, and elsewhere in the vicinity. To the best of my knowledge none of these interesting human or animal stone effigies has yet been found *in situ* by a qualified archeologist. Their authenticity, however, will be obvious to any trained observer who has the opportunity to study the specimens in the Oregon Historical Society at Portland, or in the few other collections where they occur. Several examples are illustrated here (fig. 1) primarily to indicate the occurrence in durable stone of the same art style marked by skeletal accentuation, particularly rib delineation, which marks the rarer wooden carvings preserved from the same region. Human beings are depicted either in the round (fig. 1 a, d, f), or by incision or petroglyphic technique (fig. 1 e). Many of the human effigies show clearly delineated ribs (fig. 1 a, e, f, and d faintly) but not all. Animals also occur, the owl (fig. 1 c), the

[5] Douglas and d'Harnoncourt, 1941, p. 165. Concerning Salish wood carving, see the comprehensive study now in preparation by Paul S. Wingert, of Columbia University, which should help to bring this significant art style to the attention of both artists and scientists.

FIG. 1. Stone sculptures, with ribs delineated, found on, or near, Sauvies Island in the Columbia, at the mouth of the Willamette River. (a, c–f, courtesy of Oregon Historical Society and Chicago Natural History Museum; b, after Harlan I. Smith, 1907, fig. 198a; a, 2 feet 6 inches high.)

bear (a striking example in petroglyphic technique is in the Oregon Historical Society collection), the beaver (fig. 1 b)[6] and various other forms. In regard to bird effigies the rib and wing motifs are indistinguishable (fig. 1 c) but there seems good reason to suspect an ideational connection. This art style occurs up the Columbia beyond The Dalles, on Puget Sound, and north along Vancouver Island and the British Columbia mainland to Alaska.[7] Many effigies have from one to three cupuliform cavities on the breast (fig. 1 a, c), on the top of the head, or else the body forms a bowl with head and tail projecting. This last form suggests similar bed-rock petroglyphs in the same style to be mentioned later. Quite possibly these cavities were for fish oil or other offerings.

The anthropomorphic form with ribs, sometimes surmounted by an elaborate hat or headdress, occurs commonly in petroglyphs in the same general area. An excellent example from the Wishram village at Spedis, on the Washington side of The Dalles, is shown here (pl. 2 upper). This large and deeply incised figure is said by local Indians to represent one of the spirit-guardians of the fishing places on the narrows where salmon are caught with dip nets. There are a number of these petroglyphs at this site and elsewhere in the vicinity. At high water, the Spedis examples are covered but emerge when the water drops and the dip-netting season approaches. In 1924, these "Guardians of the Fishing Places" still seemed to be held in considerable reverence by the Indians, whether as supernatural fishing aids or as family fishing-station markers was not determined. Old Wishram informants told us that the deep petroglyphs had been made by their own ancestors long ago and that they represented water animals who were the particular spirits of their ancestors. They said the numerous red pictographs and shallow petroglyphs on the rim rock in the general vicinity were made by a different, very ancient people, and no one knew how they were made. Since the latter sometimes include mounted men on horses, I suspect that people of other tribes may have made them, but perhaps not so very long ago. Sites for petroglyphs are relatively rare in the drowned river valley of the lower Columbia, but at least one large, but rather crude example of this type of ribbed human with a bed-rock mortar in the pit of its stomach occurs just back of the Fisher's Landing dock on the Washington shore above Vancouver. Similar large anthropomorphic forms, with ribs, incised on flat boulders occur as far north as the Bella Coola River on the coast of British Columbia.[8] The problem raised by such distributions is a fascinating one but only its existence and barest implications can be mentioned here.

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[6] Smith, 1907, fig. 198b. Smith also figures a cruder figure of this type from Seattle, Wash. (fig. 198a).

[7] Strong, Schenck and Steward, 1930, fig. 17, pls. 9, 11, 26. Smith, 1907, 1923, pp. 41, 57, 59, 91.

[8] Provincial Museum of Natural History, 1926, pl. III, p. C8. The occurrence of deep petroglyphic designs on salmon streams in Tlingit territory in southeastern Alaska, the prevalence of

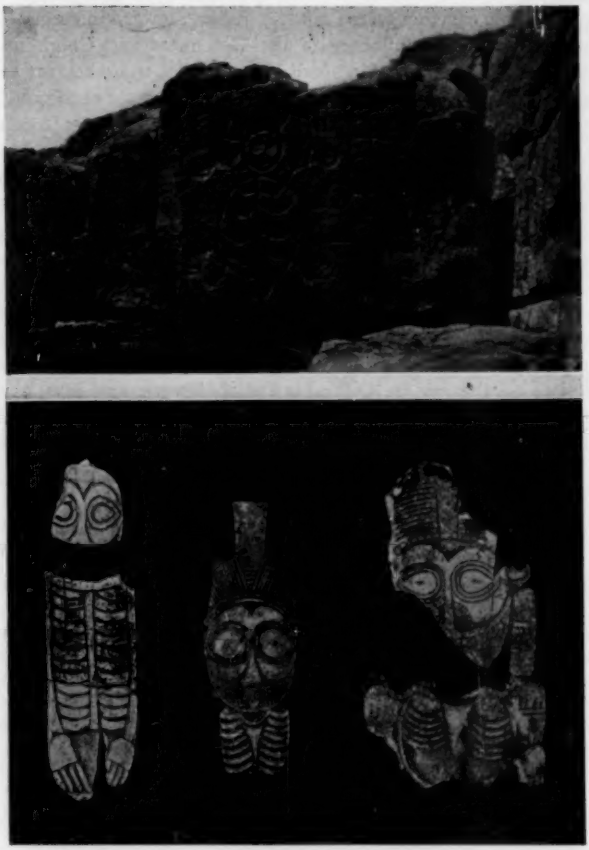

PLATE 2. (Upper) Petroglyph showing carved ribs; fishing place near Wishram Indian village at Spedis, Washington. (Carving about 3 feet in total height.)

(Lower) Carvings in bone, showing ribs, from cremation pits on Miller's Island in the Columbia, at the mouth of the Deschutes River. (After J. H. Steward, 1927, pl. 1, about $\frac{1}{4}$ natural size.)

The third class of material objects clearly characterized by this "ghost" or exposed-rib style is of bone or, occasionally, antler or slate (pl. 2, lower). These remarkable carvings were found by Julian Steward in cremation pits on Miller's Island at the confluence of the Deschutes and Columbia Rivers.[9] Since the cremation pits contain small fragments of iron they are believed to be of early proto-historic date (i.e., circa 1750) but certain of the artifacts may be of a considerably earlier period. The fragmentary carvings from these and nearby cremation pits show a remarkable blending of geometric Plateau and realistic Northwest Coast stylistic influences, but the ribbed figures with headdresses (examples shown here, pl. 2 lower) are clearly in the "ghost" style which has been shown to be represented in wood, stone and petroglyph on the lower and middle Columbia, around Puget Sound, and in western British Columbia. Thus, this "ghost" style of carving is historic on the lower Columbia since it occurs on specimens of known ethnological provenience as well as other perishable wooden effigies (pl. 1) which could not have survived much longer than since 1800; is protohistoric, since it occurs on bone artifacts from proto-historic sites on the middle Columbia (pl. 2, lower); and may also be pre-historic, and equally characteristic of an earlier period, since it occurs in, or on, imperishable stone (pl 2, upper and fig. 1), not only localized on the Columbia

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the rib motif and the beliefs of the Indians concerning such sculptures is mentioned by Keithahn, 1940, pp. 123–132. Keithahn believes that these petroglyphic sculptures were ancestral to the wood-carver's art in this region. Considering the great amount of scientific and other writing engendered by the famous art style of the Northwest Coast proper, it is strange that so little has been attempted toward determining its actual antecedents and origins. In the greater northwest the dearth of large scale, modern archeological research, closely tied in with ethnological and historical insight, has probably been the greatest preventive. See Drucker, 1943, whose recent field reconnaissance and excellent report indicate that truly scientific excavations on an adequate scale will yield important results in this promising but neglected field.

Regarding petroglyphs and pictographs of the Columbia River region, no comprehensive report has yet appeared. Cressman, 1937, entirely, albeit intentionally, ignores the highly important Columbia River region. For the middle Columbia River region, H. W. Krieger of the U. S. National Museum has much unpublished data the publication of which would have great value. The present author also has considerable data on this general area which has been lying fallow for some twenty years, despite the fact that it is, and in the past has been, repeatedly offered to and occasionally utilized by other students. Pressure of other duties now as in the past preventing full preparation of these materials, it seems worthwhile at least to point out the existence of such data and some of the wider implications they appear to contain.

[9] Steward, 1927, pp. 255–261. Also Strong, Steward and Schenck, 1930, pp. 49, 139, 140 and pl. 9. More recently a stone bowl from Miller's Island with incised designs strongly suggesting Northwest Coast designs, but still localized and unique, has been figured and analyzed by Heizer and discussed by Smith, 1943, pp. 158–160. The face designs on this bowl are strikingly reminiscent of *Tsagiglalal*, "She who watches you as you go by," a large petroglyph on the rim rock overlooking the Wishram village at Spedis, Washington. This remarkable face is figured by Curtis, 1911, and by Steward, 1937, pl. 12a.

River but also extending over a wide range stretching from northwestern Oregon to southern Alaska.

Until archaeology on something approaching an adequate scale, working from the historic into the prehistoric, has been attempted in the greater Northwest we will probably get no positive clue as to relative age of the imperishable stone effigies marked by the death, or ghost, symbol of exposed ribs. However, the occurrence of the same symbol specifically, and effigy style generally, on the wooden and bone effigies collected by George Gibbs, as well as those here figured (pls. 1, 2) and discussed, indicates that the "ghost" style, and whatever ideas or cult it symbolized persisted down into early historic times. Thus, despite the fact that inadequate archeology at the moment prevents a positive linking of historic and prehistoric manifestations in the region, the demonstrable recency of certain of the motifs in question does seem to justify an attempt to correlate them with ethnological studies and historic records.

A few quotations from Spier's penetrating analytic study of certain early historic, religious phenomena which he designates as the "Prophet Dance of the Northwest" are at least suggestive of a possible ethno-archaeological correlation in this regard.[10]

> It is the purpose of this section of this paper to show that the ultimate origin of the two Ghost Dance movements [1870, 1890] was not the Paviotso but in the Northwest among the tribes of the interior Plateau area. It can be shown that among these peoples there was an old belief in the impending destruction and renewal of the world, when the dead would return, in conjunction with which there was a dance based on supposed imitation of the dances of the dead, and a conviction that intense preoccupation with the dance would hasten the happy day. From time to time men "died" and returned to life with renewed assurances of the truth of the doctrine; at intervals cataclysms of nature occurred which were taken as portents of the end. Each of these events led to the performance of the dance with renewed fervor, only to have it fall into abeyance again when their expectations remained unfulfilled. This northwestern cult agrees precisely with the core of the Ghost Dance religion among the Paviotso, and the circumstances of its appearance among the latter parallel those of earlier date in the Northwest.
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> * * *
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> Not only is it demonstrable that the Ghost Dance parallels the Prophet Dance of the Plateau tribes and that the latter is known as long antecedent to the Ghost Dance movements, but it can be shown that the Prophet Dance was thoroughly at home in the cultures of the northwest, which cannot be maintained for the Ghost Dance in relation to Paviotso culture. It is also evident that basic elements of the complex were an integral part of Northwest Coast culture as well as that of the Plateau, which in turn argues for their antiquity.
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> One of the most striking features of Northwest Coast mythology is the occurrence of numerous tales concerning visits of the living with the dead. Many of these narrate

[10] Spier, 1935, pp. 5, 13, 16–17.

purely mythical adventures but quite as frequently they are couched as historical happenings. Similar tales are known from the Plateau tribes, although they are perhaps not quite so common. Further, the stage was set for seeming resurrections on the coast, at least at its southern end, where premature burial seems to have been not at all uncommon. It is therefore not surprising that it is this very region which provides records of men having "died" and returned from the afterworld in precise parallel to the Plateau prophets.

Several of these resurrections are known from western Washington. A Kathlamet informant dictated a tale of his grandfather's "death" and visit with the dead at the time of the small-pox epidemics (probably 1820–40). [See Footnote 12, Boas, 1901, pp. 247–251.] From the Makah there is a similar historic tale of a man who died, visited the dead, and was sent back because he was still alive. This was said to have taken place about 1858. Swan wrote that it was Makah habit to be precipitous in binding up and even burying an individual on his seeming death, without making sure that no life remained. If this was so, a return from seeming death may not have been uncommon.

* * *

It does not seem that there is any comparable constant concern with the dead in any of the areas to the south or east. It is particularly to the point to note that in the Great Basin, aside from the specific occasions of the Ghost Dance revivals, there is no such preoccupation. That is, the Basin cultures show no more than the random relations with the dead we find in the Plains or elsewhere in North America. It does not seem we can lay this negative situation to the paucity of accounts of the Basin tribes. In short, there seems to be real significance in the recurring relations with the dead in northwestern cultures, which I see as establishing a groundwork from which the Prophet Dance complex could arise.

* * *

Certainly this ethno-historic record of a "cult of the dead" that spread over the greater northwest in early historic and possibly still earlier times coincides in a rather remarkable manner with the known distribution of the tangible skeletal art motifs we have previously discussed. Concerning the exact symbolism of the Prophet Dance and its later derivatives there are a few indications that an accentuation of the bare or hollow rib motif is ethnological as well as archeological. (See footnote 3.) G. P. Murdock tells me that this is true among the Tenino, and Spier quotes Curtis concerning the "thin and raw-boned" appearance of the dead encountered by a Yakima prophet. However, this is a matter for expert ethnological consideration and is offered here merely for what it may prove to be worth. What is most significant is the fact that Spier, by ethnological analysis, demonstrates that the roots of the historic ghost dances (1870, 1890) can be traced back another century at least, and that now archeology suggests that they may be still older.

However deeply rooted this concern with death and the return of the dead may have been, it is worthy of note that the tragic manner and speed with which the native population of the lower Columbia valley disappeared should

certainly have accentuated this feeling. Lewis and Clark, the first white men to come down the Columbia, noted many abandoned villages and, already, many evidences of white contact by way of the Pacific. They record two or three cases of gonorrhea and six of syphilis on the lower river but state that, while these diseases occurred, the number of natives affected was inconsiderable. Less than ten years later, in 1814, Alexander Henry notes the rapid spread and terrible prevalence of these diseases that occurred after 1810. This was the date when white traders established themselves at the mouth of the Columbia, bringing such infections in "not only from New York but also from the Sandwich Islands."[11] One generation of whites practically saw this sad process of extinction completed. Writing in 1906, the father of the present writer states:

> Queen Sally, of Cathlamet, was the oldest living Indian on the Lower Columbia in the late fifties and early sixties, and her memory went back easily to the days of Lewis and Clark when she was a young woman old enough to be married, which, with the Indians, meant about the age of fourteen. Seventy years is extreme old age for an Indian, and especially for an Indian woman, but Queen Sally was all of this. Judging from her looks she might have been anywhere in the centuries, for never was a more wrinkled, smoke-begrimed, wizened old creature. Princess Angeline, of Seattle, was a blooming young beauty beside her.
>
> It gave one a far-away feeling, in regard to the event not warranted by the years that had passed, when from the cliffs above Cathlamet she pointed out the spot where the canoes of Lewis and Clark were first seen. She said the Indians had been on the watch for them for several days, as news had come by Indian post of the strangers from the East. Lewis and Clark with their party came in the afternoon or evening, and were met by the Indians in their canoes at or a little above the modern town of Cathlamet and escorted to the Indian village, which was then on the slough below Cathlamet, at about the point where the saw mill now is. How long they stayed here she could not clearly tell. It was evident she confused their westward and eastward trips and also their winter stay at Clatsop with their stay at Cathlamet village. Twenty-five miles to wandering Indians is a bagatelle of too little importance to be considered in fixing a locality. It was a time of feasting, wonderment and council making. Lewis and Clark were doubtless weary of Indians by this time, but the strange sights they saw will never be seen again."[12]

Thus, while my father grew up in close association with many Indians at Cathlamet village, some of whom remembered the visit of Lewis and Clark, the writer, who spent his early years on the Columbia has, to the best of his knowledge, never seen a Chinook of the lower river.

[11] Coues, 1897, Vol. II, p. 836 and fn. 9.

[12] Strong, 1930, pp. 5–7. Thomas Nelson Strong was born at Cathlamet in 1853 and died in 1927. Boas, 1901, while collecting his *Kathlamet Texts* in 1890 and 1891, reports finding only three speakers of the Kathlamet dialect, all living in other towns. J. P. Harrington, however, tells me that within the last few years he has located several other speakers in the state of Washington.

The passing of the natives of the lower Columbia, and its psychological concomitants, is thus described by one who knew the Indians well.[13]

Another curious question has to do with the scanty native population of Western Oregon and Washington when first known by the white men. The range was limitless and food abundant beyond measure. The country could have supported easily five times the number of native people that were on it. These Indians always claimed that they were once a populous and powerful people, but that in some way they had provoked the Divine anger and been destroyed, and this claim is undoubtedly based upon fact, and on this question, although there are uncertainties regarding the manner of the decimation of the Indians of the Willamette and Lower Columbia Rivers, we have something definite to go on. This decimation began before the first white settlers came, and was largely finished before 1830. None of the histories give any idea of the number of Indians who inhabited this region before historic times, and this can only be conjectured, but it is certain that once a commensurate Indian population filled Western Oregon from the Cascade Mountains to the Pacific Ocean. Every aged Indian told stories of a time when the rivers were lined with villages and floated many canoes.

At Marr's Landing, about three miles below Castle [Beacon] Rock, on the Columbia, the river has in the last few years been washing away what is known as the island, and has uncovered the site of old Indian campfires. These stretch in a long line up and down the beach. They are covered with two or three feet of loam, and on this fir trees a hundred years old have grown. As many as fifteen or twenty stone hammers have been found about a single fireplace, and these old charred fires are preserved as they were 200 years ago. One pathetic little relic found amongst the big stone hammers was a tiny little hammer and pestle, evidently play-things of an Indian child. On Archer Mountain, a mile or two west, are what appear to be ancient fortifications that would have required many warriors to man. No village of this magnitude was known there by white men. In the days of Lewis and Clark there was only a scattering settlement near Castle Rock, and a migratory trading band at the Cascades.

The Indian flint factory at the Clackamas River suggests a large population, and Cathlamet was always a greater city of the dead than of the living. Between the Elokomon and the Skamokawa the sloughs were lined with the burial canoes of the dead, and as only distinguished men were so buried, this stood for a very large population, probably greater than that of the Bella-Bella Indian Village in British Columbia. These canoe burials were ancient to say the least. Cedarwood is almost indestructible, and no living Indians knew the name or lineage of the dead or resented the resurrection that the white children accomplished in searching for Indian ornaments. They tumbled the bones out of the bed of loam and leaves that had gathered over them, and they were the bones of a hundred years gone. In sport the children put them together and speculated upon what manner of men they were, and the Indian children joined in the game, for the dead were the old, old people.

Below the Indian village the ground was black and the plough turned up countless skulls and bones with flints and Indian arrowheads, bespeaking long occupation and a numerous population.

[13] Strong, 1930, pp. 50–57.

Long before 1800 the Indian had evidently reached the height of his power and prosperity, and when the white man came was already on the way to extinction.

The waning of the Indian power of the Lower Columbia is shrouded in mystery. Young Indian girls told the story of it in hushed whispers, and the old Indians spoke of it reluctantly. Had the Death Angel come in bodily form they could not have been more impressed. The wail for the dead, so they said, was heard all along the rivers, and no one even hoped for life when the slaughter was on.

The Indians named the chief instrument of destruction the "Cole sick." With the white man came the smallpox and the measles, but the "Cole sick" was neither of these. About 1820 and 1830 epidemics of the old disease swept among the remaining Indians, and historians are puzzled to give it a name.

One suggests that fever and ague came with the settlers, but the Valley of the Columbia was never a fever and ague country and the pioneers, however malaria stricken at the beginning, must have been thoroughly disinfected by their long trip across the plains. Others say that the turning up of the soil by the Hudson's Bay people at the farms at Fort Vancouver released malaria from the soil and this caused the epidemic, but the disease was here before the farms, and it was impossible that a disease which raged over hundreds of square miles could have come from so trivial a cause. It may have been the modern la grippe striking an unprotected people. Whatever it was no more potent angel of death ever visited an afflicted people.

The white man had no need of war or violence in his dealings with these Indians, nor did he employ them, for the "Sahalee Tyee," the Indian god, had struck before him.

The foregoing description of rapid tribal and cultural breakdown, accompanied by extreme population decline, on the lower Columbia River in the far west, according to at least one interpretation, finds a close parallel in the last stages of native cultural history in the Mississippi River region to the east. Further, these manifestations, on both seaboards, seem to have been accompanied by somewhat similar, although differently derived, psychic and cult manifestations. In this regard a recent archeological analysis and anthropological summary of the region of the eastern United States concludes:[14]

Toward the end of this stage it can be seen that a marked decrease in population is occurring. In areas where sites of the early and late Middle Mississippian cultures must have been numbered in the thousands, not an Indian was to be found in the latter part of the seventeenth century when the French and English explorers entered the region. This is particularly true for the Mississippi and Ohio valleys and for much of the country covered by the Tennessee-Cumberland and Lamar cultures.

During the Temple Mound II stage there appears to have arisen a curious cult which shows little relationship to anything which has previously transpired, and which spread rapidly over the entire Mississippi Valley area, although most common in the south. The paraphernalia from which the presence of this cult is deduced show a high degree of similarity all over the area. Included are such items as conch shell masks marked with the winged or "weeping" eye symbol; copper and shell pendants with circles and crosses engraved, repousse, or with background cut out; engraved

[14] Ford and Willey, 1941, pp. 357–359.

conch shells or thin copper plates on which are depicted dancing figures wearing eagle masks, carrying a human head in one hand and a peculiar shaped baton in the other; shell gorgets showing fighting turkey cocks or rattlesnakes; monolithic stone axes; large stone batons; the horned and winged rattlesnake engraved on circular paint palettes or on pottery; and fairly large stone figures with negroid faces and characteristic arrangement of hair in two rolls on the top of the head.[15]

Some items of this paraphernalia have been found at nearly every site of this period which has been thoroughly investigated. However, some of the larger centers have become well known for the quantity of these materials which they have yielded. Outstanding are the Kincaid and Angell sites near the mouth of the Ohio River, the Etowah site in north Georgia, Moundville in central Alabama, and Spiro in eastern Oklahoma. Although the cult which spread these objects probably flourished about 1600, there are some finds showing that certain features, at least, lasted until after 1700.

The suggestion of Mexican influence in these objects has been recognized.[16] Aside from that, there is a great temptation to interpret these objects as evidence of some sort of religious revival quite similar in purpose to the well known Ghost Dance cult of the Plains. This, also, may be the evidence of a reaction to a rapid decimation of the population of a formerly vigorous and thriving culture.

Early contacts with Europeans may have brought among the native peoples highly contagious and, to them, very fatal new diseases such as measles, smallpox, cholera, etc. DeSoto's party of exploration across the south, 1539–1543; the early settlement of the Virginia coast, 1607; Spanish Missions and settlements in Florida and Georgia following 1520, may all have served as foci of contagion. The early disappearance of the native peoples from Florida and the vicinity of the Atlantic coastal settlements is well attested by the historical sources.[17]

The same cause, the impact of highly destructive epidemics, may account for the change of population centers noted above. In the thickly settled valleys of the larger rivers the epidemics would have been most destructive. The surviving remnants abandoned the old villages and gathered in new settlements back in the hills. Perhaps the unrest of the period is reflected in the compact settlements and extensive use of stockades.

Much of the widespread scattering of peoples, notably the Yuchi, may have occurred at this time. These same factors may also account for some of the curious mixed villages described bout 1700 such as the Yazoo, Tunica, Coroa, and Ofogoula, gathered into a village on the Yazoo River in Mississippi. It is suggested that the native peoples and culture, as described about 1700, were, for the most part, already a broken people with a declining culture.

While agreeing on the late occurrence of this complex of exotic archeological motifs in the Southeast, and demonstrating that their apparent Mixteca-Puebla cast may possibly be explained as a direct transmission through the numerous Mexican Indians brought to Florida by the De Luna expedition in 1559, Griffin doubts that the "death symbols" and other motifs had any connection with a "ghost dance" or "death cult." He states: "Some students have

[15] Waring and Holder, 1945. [16] Phillips, 1940, pp. 349 ff. [17] Swanton, 1935.

viewed these art styles as the expression of a religious revival brought about during a fanciful period of decline and decay of southeastern culture.[14] In the writer's opinion the art styles resembling Mexican forms are a part of the culture at the highest aboriginal level of accomplishment and represent not a stage of retrogression in the Southeast but its apogee."[15]

This difference of opinion will undoubtedly be settled by more exact archeological correlations and, quite possibly, by new (or newly interpreted) historical and ethnological data. In this regard it should be noted, as the archeological record of the Pawnee, Arikara and Mandan in the Great Plains so strongly indicates, that the protohistoric period of first contact with an alien culture may mark both the apogee of the autochthonous culture and the moment of its rapid decline. So far as I am aware, no ethnologist has yet pointed out ethno-historical evidence of a "Death" or "Prophet Dance" in the Southeast, as Spier has done for the greater northwest. More data on these problems will undoubtedly be forthcoming. In the meantime, the present writer finds the parallelism, here so sketchily indicated, between the psychic, cultural and demographic manifestations of the closing stages of native culture on the greatest eastern and greatest western waterways in the territory of the United States a very suggestive one.

As yet such correlations are not clearly established in either the Southeast or the greater northwest. However, it is most encouraging that ethnology and archaeology, in some areas of the New World, are at last converging so closely that such significant problems emerge almost of their own volition. Thus, in the Southeast the possibility of the occurrence of a protohistoric revivalistic cult with Mexican resemblances has been first indicated through archeological techniques, whereas, in the greater northwest, ethno-historical analysis clearly indicates the early and wide spread of a Prophet Dance concerned with death and revival, long anterior but ancestral to the later well-known Ghost Dances. As the foregoing discussion attempts to point out, there is a good chance that archeology in the Northwest—beginning in contact sites and working backward in time—can objectively correlate the material phenomena here interpreted as indicating a "Ghost Cult" with those social phenomena which Spier calls the Prophet Dance. If this correlation is established and archeology demonstrates that these "death and revival" phenomena in the west are actually pre-Columbian, considerable re-orientation in current hypotheses concerning the causation of such messianic or revivalistic phenomena will be necessary.

On the other hand, if such correlated ethnological and archeological researches in the west and in the east confirm the present protohistoric or first contact dating for such revivalistic phenomena, in conjunction with rapid depopulation and cultural breakdown, other important world-wide problems of

[15] Griffin, 1944, pp. 299–303.

traumatic culture contact between alien peoples are posed. Few of the current population estimates for aboriginal America seriously consider this obscure but obviously important factor, yet archeological and historical research in the Southeast, the Great Plains and, apparently, the greater northwest, indicates that just prior to the period of recorded French and English exploration the native populations, particularly the sedentary riparian peoples, had already been largely displaced and apparently nearly exterminated. If we are to understand the historical and biological causes for such phenomena, as well as the demographic and psychological results therefrom, it is obvious that we must have many more penetrating studies wherein ethno-historical analysis and synthesis are fused with historically creative archeology. Such correlated studies alone can establish a specific temporal framework for such nonliterate societies within which the demographer, epidemiologist, and art historian, as well as the sociologically or psychologically minded student of culture dynamics and personality, can at last operate with historically calibrated tools.

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IRRIGATION AGRICULTURE AND NAVAHO COMMUNITY LEADERSHIP: CASE MATERIAL ON ENVIRONMENT AND CULTURE

By ESTHER S. GOLDFRANK

THE Navaho, an Athapascan-speaking people, probably entered the Southwest some five hundred[1] to one thousand[2] years ago. "There is," writes Amsden, "good reason to believe the tribe centered in early historic times in the northeastern portion of its later broad domain, with the San Juan River marking its northwesterly limit and the foothills of the high mountains lying along the present Colorado-New Mexico boundary as a barrier to the northeast. Southward lay the chain of Pueblo villages of the Rio Grande valley, westward a vast empty area which the ambitious tribe was rapidly to make its own."[3] Today, the Navaho reservation encircles the Hopi country in a broad girdle, 24,000 square miles in area, parts of which extend somewhat beyond the northeastern boundaries of Arizona into the adjacent states of New Mexico and Utah.[4]

The Navaho have been called a pastoral people,[5] a nomadic people,[6] and a nomadic people who "depend comparatively little upon agriculture."[7] However, A. L. Kroeber, who classes them as inter-pueblo,[8] remarks:

> Both Gladys Reichard and W. W. Hill tell me that maize is the Navaho staple food and that most Navaho farm."[9] . . . "If so, the body of ethnologic literature on the Navaho stresses so important a fact surprisingly lightly."[10]

So rarely, indeed, has the point been made by ethnologists that it seems desirable to quote Hill rather fully:

> In connection with moving sheep to the mountains for summer pasturage and the winter move of the household to the foothills for fuel, there has grown up a current belief that the Navaho are nomadic. This is emphatically not the case. While they may be only partly sedentary, they are not in any sense a nomadic people. Whatever their older practices, the Navaho have been since historic time primarily sedentary agriculturists. Furthermore, they consider themselves primarily farmers and as such belonging to a certain restricted area.[11]

Obviously, the area occupied supports both agriculture and herding,[12] and obviously, also, the Navaho,[13] like the Pueblos,[14] practised both. But no doubts

[1] Hodge, 1937, p. 111.

[2] Most specifically, Hall, 1944. See also for somewhat later dating, Mera, 1935, pp. 8, 34; *idem*, 1938, Judd, 1940, pp. 436–437; Parsons, 1939, Vol. II, p. 1039. [3] Amsden, 1934, p. 126.

[4] Hack, 1942, maps on pp. 3 and 16. [5] Bunzel, 1938, p. 330.

[6] Hoover, 1931, pp. 429–445. [7] Goddard, 1931, p. 145. [8] Kroeber, 1939, p. 35.

[9] *Ibid.*, p. 179. [10] *Ibid.* [11] Hill, 1938, p. 18.

[12] Linton, 1936, pp. 387 ff. makes the point for agriculture as against hunting and food-gathering which were competing economies in pre-Spanish times.

[13] Most particularly, Hill, 1938, p. 18; Reichard, 1928, pp. 7, 155 ff.; Kimball and Provinse, 1943, pp. 18, 19; Leighton and Kluckhohn (MS); Sapir, 1936, p. 231; and Collier, 1939, 1939a, 1940.

[14] Among many others, Beaglehole, 1937, pp. 33 ff., and pp. 49 ff; and Parsons, 1939, Vol. I, pp. 109, 110, 150, 402; Vol. II, p. 892, 1110, 1115.

are ever expressed on this account regarding the agricultural base of Pueblo economy, for in their case the situation is clear. The contradictory statements on the economic base of Navaho society, on the other hand, reflect the complexities of their historical development. Only when this is examined over time, is the role played by agriculture—and irrigation agriculture—disclosed.

HOW AGRICULTURAL WERE THE NAVAHO?

Sheep, goats, and horses were introduced by the Spaniards who first entered the Southwest in 1539. Navaho sheep are mentioned in the chronicles of the 18th century;[15] their bones have been uncovered in archaeological remains of the same period,[16] and there is reason to believe that they were known to the tribe considerably earlier.[17] But it was not until 1868, when the Navaho returned from Fort Sumner, after a four-year involuntary stay, that herding became significant; 15,000 sheep and goats were then issued to the tribe,[18] and "their increase was phenomenal."[19] By 1879, Navaho sheep numbered 700,000 head;[20] by 1942, after a program of reduction had been introduced, the tribe still had "750,000 sheep units. . . . 583,569 of which were within the boundaries of the Reservation."[21]

While sheep-herding is well documented for the Navaho, history and archaeology are no less convincing regarding their agricultural activities. In 1630, Benavides commented in his memorial: "But these of Navaho are great farmers, for that is what Navajo signifies—'great planted fields'."[22] Except for Zarate Salmeron who mentions the Apache Indians de Nabaju in passing,[23] this is the first description of the tribe known to us. Something over a century later Rabal, governor and captain general of New Mexico, submitted to his Spanish sovereign the reports of twelve observers sent by him into the Navaho country. Hill, in summarizing the reports writes:

> All the deponents mention farming and the type of crops raised, the making of basketry, weaving of cloth, and type of habitation. All but one speak of military activities. Less prevalently recurring references include those to horses, sheep, goats, and cattle. . . .[24]

A few excerpts from the reports follow: "In this unsettled country there are three rivers with little water and three small springs also fresh; . . . it is poor soil because it is mountainous . . . 10 leagues distant there is a very large valley long and wide and good soil where they do most of their seasonal plant-

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[15] Hill, 1940a, pp. 395 ff.

[16] Keur, 1941, p. 46: "Sheep bones were numerous and scattered, indicating widespread use."

[17] *Ibid.*, p. 2. [18] Commissioner of Indian Affairs, 1870, p. 145.

[19] Kimball and Provinse, p. 20. [20] Commissioner of Indian Affairs, 1879, p. 115.

[21] Leighton and Kluckhohn, (MS).

[22] Benavides, 1916, p. 44. According to Harrington, 1940, p. 518 the term "návahuú" is Tewan (nava, "cultivated fields," "milpa," "garden"; huu, "large arroyo").

[23] Lummis, 1900, Vol. XII, p. 183. Translation of Relación of Zarate Salmeron.

[24] Hill, 1940a, p. 396.

ing."[25] Another witness, penetrating the province almost thirty leagues, found a "wooded country with many gullies without any rivers but with some pools of running water in which valleys the Indians plant corn, beans, pumpkins and watermelons as seasonal crops."[26] Another speaks of water that "is running and . . . from it some small vegetable farms are irrigated. . . ."[27] Some time later, this same witness notes "that in some of the valleys they found holes with rain, that was dammed by the Indians by sand dikes."[28]

In 1849, three years after the Navaho came under the jurisdiction of the United States, James S. Calhoun, Superintendent of Indian Affairs in New Mexico, writes upon entering the Cañon de Cheille: "The Navajoes . . . have extensive fields of corn & wheat—fine peach orchards, and grow quantities of Melons, Squashes, Beans and Peas, and have immense flocks of sheep."[29] Three years later, John Greiner, Acting Superintendent, reports: "The Navajoes are attending to their farms, and behaving like good people ought to do. Sixty of them under Sandoval their captain arrived here last week, and entered a complaint against the Laguna Pueblo Indians for taking water needed by them to irrigate their lands."[30] In 1916, H. E. Gregory estimated that 20,000 acres on the Navajo Reservation were cultivated by flood-water farming.[31] In 1941, after considerable government outlay, 16,000 of the 40,000 acres devoted to agriculture were irrigated from a permanent water supply.[32]

Archaeology, while less explicit regarding the manner of cultivation, clearly reveals Navaho familiarity with agriculture during the early historic period. About the time that the Rabal reports were being made, numerous Navaho sites at Big Bead Mesa in New Mexico (one containing as many as 29 and another 32 hogan ruins) were occupied by "an agricultural people who utilized meat both of domestic and wild animals."[33] Navaho sites in Upper Blanco and Largo Canyons occupied at approximately the same period[34] yielded "quite a number of corncobs."[35] Farmer reports: "Two basic types of metates were found, a slab and a trough open at one end[36]. . . . Manos were of two general types, oval and rectangular."[37] The earliest hogan site in this same general area which contained additional Navaho remains in all states of preservation was dated by the tree-ring method as early as 1540—"a time verging on the prehistoric."[38]

Less certain as to origin, but most suggestive, are the finds of Mera and Hibben in the Gallina area (40 miles airline south southeast of Governador)

[25] *Ibid.*, p. 401. [26] *Ibid.*, p. 402. [27] *Ibid.*, p. 401. [28] *Ibid.*, p. 402.

[29] Calhoun, 1915, p. 32. No estimate of the number of these sheep is given, but eighteen years later, the Report of the Department of Indian Affairs for the year 1867 (p. 203), reads, "The number of animals owned by the Navajos on the reservation is as follows: horses, 550, mules 20, sheep 940, goats 1,025." It is more than probable that others survived in the distant hills in which many of the tribe took refuge. [30] *Ibid.*, p. 530. [31] Gregory, 1916, p. 104.

[32] Ricketts and McPhee, 1941, p. 9. [33] Keur, 1941, p. 45. [34] Farmer, 1942, p. 71.

[35] *Ibid.*, p. 73. [36] *Ibid.*, p. 71. [37] *Ibid.*, p. 72. [38] Hall, 1944, p. 100.

of a "pointed bottom pottery . . . difficult to distinguish from Navajo pottery," which "occurs nowhere else in the Southwest at this time (1100–1250 A.D.)."[39] E. T. Hall, in "reconstructing" the early history of the Governador region, also notes that pointed pottery appeared in this area at about the time the Pueblo-like peoples who inhabited it were forced out (between 900 and 1000 A.D.) presumably by nomads.[40] "These nomads," he adds, "may have been the ancestors of the Navajo first found in the adjoining Governador . . . 300 years later."[41]

Since 1630, or for more than three hundred years, Navaho agriculture has been reported in the literature; and archaeology has definitely pushed back the horizon another hundred. But, on the basis of the accumulating evidence, it may be reasonably assumed that the Navaho were in contact with "Pueblo-like" peoples centuries before the Spaniards touched the shores of America.

While archaeological evidence to date can only suggest prehistoric contact between the Navaho and certain sedentary settlers in the Southwest, Navaho myth and ritual are so replete with agricultural concepts and detail that the assumption of a long-standing and intimate knowledge of husbandry seems more than justifiable.[42] There is no need here to give examples of the endless use of pollen, corn, and cornmeal that fill the pages of recorded Navaho myth and ceremonial, but it is most significant that the tribe's First Man was created from white corn, their First Woman from yellow corn.[43] According to legend, these crucial events took place before the Navaho emerged from the fourth underworld; and it was also there that they first met the Pueblo peoples.[44] This last may test our credulity, but it adds considerable weight to the belief that contact between the groups has been of long duration. Even if doubt is felt as to the time and place of meeting, that meeting was evidently a most important one for the Navaho. It is described with unusual explicitness in their origin myth:

After the flood had abated,

two couriers were sent to the north. When they got back they said they had found a race of strange men, who cut their hair square in front, who lived in houses in the ground, and cultivated fields. These people were engaged in gathering their harvest and gave them food to eat. . . . The day following . . . two of the newly discovered race—Kisáni (Pueblos) they were called—entered the camp and guided the exiles to a stream

[39] *Ibid.*, p. 101.

[40] *Ibid.*, p. 102 ff. For an illuminating discussion of the reasons for abandoning prehistoric pueblos, see Linton, 1944.

[41] *Ibid.*, p. 103.

[42] If the Navaho cultivated the soil only after the Spanish conquest it seems difficult to understand why horses and sheep, which were known to have been acquired at that time, are so rarely mentioned in their ritual and ceremonial chants.

[43] Matthews, 1897, p. 69.

[44] *Ibid.*, p. 68; cf. also Klah, 1942, p. 45. Here Begochiddy creates both the Navaho and Pueblo peoples. According to Haile, 1938, p. 16, "First Man created the first Pueblo man and woman by stepping over his magic bundle."

of water . . . they went over the stream and visited the homes of the Kisáni. . . . The Kisáni gave the wanderers corn and pumpkins to eat, and the latter lived for some time on the food given them daily by their new friends. . . . The land of the Kisáni had neither rain nor snow; the crops were raised by irrigation.[45]

This agricultural technique must have greatly impressed the hunting and food-gathering Navaho, for the narrative continues:

The primal pair had in all five pairs of twins, the first of which only was barren, being hermaphrodites. . . . This numerous offspring married among the Kisáni and among those who had come from the lower world, and soon there was a multitude of people in the land. . . . These descendants of First Man and First Woman made a great farm. They built a dam and dug a wide irrigating ditch.[46] But they feared the Kisáni might injure this dam or their crops; so they put one of the hermaphrodites to watch the dam and the other to watch the lower end of the field. The hermaphrodite who watched the dam invented pottery. He made the first plate, a bowl, and a dipper, which were greatly admired by the people. The hermaphrodite who lived at the lower end of the farm invented the wicker waterbottle. Others made, from thin split boards of cottonwood, implements which they shoved before them to clear the weeds out of the land. They made also hoes from shoulder-blades of the deer and axes of stone. They got their seeds from the Kisáni.[47]

The final statement becomes doubly convincing in view of Sapir's linguistic researches. He writes: "The Navaho word 'corn', *nà· dą́*—can be analyzed with great probability into an older 'food of the enemy', 'Pueblo food. . . .'"[48]

In the face of such extensive evidence, both internal and external, concerning Navaho agriculture, it may indeed be asked how the body of ethnologic literature stresses this agricultural background "so lightly." True, Navaho archaeology is only in its infancy, but historical records, even when they were easily available, have rarely been consulted. Yet it is these records that furnish the key to the enigma.

After the farming fiasco at Bosque Redondo (Ft. Sumner),[49] and after the Navaho's return to their old homes in 1868 "too late to plant,"[50] and after a severe frost in 1870 "making ice on our acequias,"[51] the desultory decade of 1870–80 ended with "a succession of two years drought" which "placed these Indians in a very bad condition. . . ."[52] The serious deepening and widening of stream channels (arroyo cutting) at that time further accelerated the already advanced abandonment of cultivated fields.[53]

[45] Matthews, 1897, p. 68.

[46] *Ibid.*, p. 69. For further agricultural techniques, see *idem*, 1902, pp. 187 ff.

[47] Matthews, 1897, p. 70. [48] Sapir, 1936, p. 231.

[49] For the years 1865, 1866 and 1867, see U. S. Report of the Commissioner of Indian Affairs, 1867, p. 190; for the next year see U. S. Report, 1868, p. 161.

[50] *Ibid.*, 1868, p. 163. [51] *Ibid.*, 1870, p. 149. [52] *Ibid.*, 1880, p. 253.

[53] Bryan, 1929, p. 453; cf. also *idem*, 1925, pp. 338–344.

On the other hand, this very decade saw an amazing increase in Navaho flocks. In 1870, the Superintendent of Indian Affairs of New Mexico wrote: ". . . nearly a year ago, I purchased for these people 14,000 sheep and 1000 goats."[54] By 1879, the sheep alone numbered 700,000. So impressive were the flocks that in 1883 the agent, even while noting the unsatisfactory character of Navaho agriculture, said of his charges: "They are a purely pastoral people, and necessarily so."[55] An almost identical statement was made some thirty-four years later. H. E. Gregory, a geologist, who in 1916 wrote a most comprehensive report on Navaho water-supply and irrigation, says in a publication that appeared the next year: "The Navaho is a stockman, a nomad who follows his flocks in search of forage and water."[56]

But however justified the emphasis on herding in the last thirty, forty, or fifty years, Navaho institutions, if they are to become comprehensible at all, must be studied as fully as possible in the light of their total known past. The present examination of Navaho agriculture and community leadership is offered as one small step in this direction.

THE NAVAHO COUNTRY

Despite variations in altitude,[57] temperature,[58] wind,[59] and soil,[60] H. E Gregory, writing of this region, observes: "Topographic features of all grades show the influence of aridity."[61] Along the lower course of the Little Colorado, precipitation probably does not exceed 3 inches a year;[62] in the San Juan valley, Aneth receives 4.96 inches, Fruitland, sixty miles up stream, 6.89; in the center of the reservation, Chinle registers 10.92, Keams Canyon, 10.94, and St. Michaels 12.80. Navajo Mountain, where vegetation suggests a rainfall of more than 20 inches, probably marks the upper moisture limit.

But such averages are far from stable. From year to year, rainfall on the reservation may range between half the normal and twice the normal.[63] Furthermore, seasonal distribution is strikingly uneven, the maximum precipitation, 37 per cent, falling in July, August and September, and only 12 per cent in the three preceding months, the driest of the year.[64] Winter and Fall respectively account for 25 and 26 per cent of the annual precipitation.[65]

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[54] Commissioner of Indian Affairs, 1870, p. 145. [55] *Ibid.*, 1883, p. 120.

[56] Gregory, 1917, p. 14. For a similar inconsistency see Leighton, 1944, which contains numerous references to agriculture (particularly p. 14) and opposite p. 18 exhibits a map in which the Navaho are labelled "Shepherds."

[57] Gregory, 1917, p. 21. "The extremes of relief are Navajo Mountain, 10,416 feet, and the mouth of Little Colorado River, 2,800 feet above sea level."

[58] *Ibid.*, p. 63 ff. "The greatest range (127°) is at Holbrook where a July temperature of 106° is offset by the low December record of 21° below zero" (p. 66).

[59] *Ibid.*, pp. 67 ff. [60] *Ibid.*, pp. 68 ff. [61] *Ibid.*, p. 22.

[62] *Ibid.*, pp. 59 ff. for this statement on rainfall and those that immediately follow.

[63] *Ibid.*, p. 60. [64] *Ibid.*, p. 61.

[65] *Ibid.* Exceptions are Tuba, Aneth and Hite.

The season of least rainfall, April to June, is also the growing season of the basic crops.[66] As much as half an inch of rain per month for most parts of the Navaho country is unusual during this period,[67] and "during many years the combined precipitation of these three months is less than one-half inch."[68] Meager as this supply is, the plants obtain only a portion of it, "for evaporation is most effective during the clear hot dry days of early summer."[69] Gregory concludes:

> The moisture in the ground, supplied by the rains of winter and supplemented by the scattered showers of spring, is sufficient to allow seeds to germinate and to send their stalks above ground, but is insufficient to bring a crop to maturity. The rainfall of July becomes therefore the critical climatic factor in the life of the Navajo."[70]

NAVAHO AGRICULTURE

The Navaho planter is well aware of his precarious situation. To be sure, at Chinle, with its abundant underground water, total crop failure is rare,[71] but "for a large part of the reservation, corn, without irrigation, fails to mature every second to fourth year."[72] It is not surprising then that in the Navaho country "the use of flood waters for irrigation has been a feature of agricultural practice . . . for perhaps a thousand years."[73]

Ethnologists have not always understood that flood-water farming as practised in our Southwest both in ancient and modern times is, in most cases, a form of irrigation. The difficulties of discernment have been pointed out by Kirk Bryan, professor of geology and geography: "The usually clear distinction between irrigated and unirrigated farm land breaks down in the consideration of marginal lands irrigated by flood water";[74] for, he explains, "fairly well designed irrigation systems . . . to utilize water during the time of heavy runoff in the spring" may be constructed in some areas, while in others, "no regular system of diversion or of conveyance of water is maintained."[75] However, the increasing evidence of extensive systems of diverting ditches, check dams, and boulder breaks at numerous ancient sites in the Navaho and adjacent country[76] would seem to confirm Bryan in his opinion that "flood-water farming . . . was more prevalent in the early days of settlement than at present,"[77] and, it might be added, more elaborate.

But even today in their marginal lands, so Gregory reports:

> The Navajos sometimes endeavor to direct the floods and to prevent excessive erosion within the fields by constructing earthen diversion dams a few inches to a foot or more in height—dams which require renewal each season. Along the smaller washes and in

[66] *Ibid.* [67] *Ibid.* [68] *Ibid.* [69] *Ibid.* [70] *Ibid.*, pp. 61 ff. [71] *Ibid.*, p. 63.
[72] *Ibid.*, p. 62. [73] *Ibid.*, pp. 103 ff. [74] Bryan, 1929, p. 444.
[75] *Ibid.*, cf. also Hill, 1938, p. 24.
[76] See particularly for a recent summary, Stewart, 1940, *passim;* also Stewart and Donnelly, 1943, *passim.* [77] Bryan, 1929, pp. 444, 453; cf. also Gregory, 1916, p. 105.

places where the slope of the ground is moderate the common practice of the Navajo is to build a series of check dams 50 to 200 feet apart and 2 to 5 feet high, which not only retard the run-off but also serve to form temporary ponds for stock watering.[78]

Waters from perennial streams have also been diverted by dams and led onto adjoining flats. While Rabal, in 1753, mentions irrigation from running water, and Greiner, a hundred years later, also refers to it, comparatively little is known concerning the extent of this type of water-control for the early history of the Navaho. However, in 1916, Gregory wrote:

> Standing Redrock Creek, in Redrock Valley, is diverted to water more than 100 acres of cornfield. Lukachukai Creek supplies, through a series of short ditches, water to cover 300 to 400 acres and makes this place one of the most prosperous Navajo settlements in Arizona. Ditches were also noted along the Tisnasbas, Nazlini, and Wide Ruin washes, and at several other localities on the reservation.[79]

In 1941, the Navajo Service reported that "eight new irrigation projects have been developed and cropped and 17 old Indian projects have been improved by the government since 1933."[80] In addition, work on 17 more was being planned for, bringing "to 69 the number of active irrigation projects on the reservation." At Shiprock, alone, 3,000 acres were irrigated from a permanent water supply.[81]

IRRIGATION—COOPERATION—LEADERSHIP

Considerable space has been devoted to demonstrating, first, that, for historic time and probably much longer, agriculture has been the basic subsistence economy of the Navaho—and, second, that this agriculture depended primarily on irrigation, more particularly on flood-water irrigation. These facts, once established, make it relevant to ask: "What patterns of community organization and leadership emerged among the Navaho under the stress of their semi-arid environment?"

In recent years increasing attention has been given to the effect of large-scale water control on the structures of pre-industrial agrarian societies such as Egypt, India, China, Mexico, and Peru.[82] Whether this control was for purposes of irrigation proper or for the prevention of ravages from flood, cooperation—tribal, regional or national—was necessary for the building and maintenance of dikes, canals, ditches, storage basins, etc., and directing centers of

[78] Gregory, 1917, p. 104. [79] *Ibid.*, p. 105.

[80] Ricketts and McPhee, 1941, p. 13.

[81] *Ibid.*, p. 12. According to Leighton and Kluckhohn (MS) this has since been increased to 9,849 acres.

[82] Wittfogel, 1932, for Peru, pp. 589 ff.; for Egypt and Babylonia, pp. 598 ff.; *idem*, 1931, for Egypt and Mesopotamia, p. 86; for India, pp. 295 ff.; for China, pp. 298 ff.; *idem*, *Oriental Society in Asia and Ancient America* (in preparation). Cf. also Weber, 1923, p. 64; Rostovtzeff, 1930, pp. 18 ff.; Albright, 1940, p. 104; Breasted, 1935, p. 53. For the New World, Sapper, 1931, pp. 180, 246; also Kosok, 1940, pp. 169–178, and Strong, 1943, pp. 19, 24, 40.

authority arose. What has been strikingly obvious in the great "Oriental" civilizations (witness the extraordinary centralization of power in the hands of the monarch and his supporting bureaucracy on the one hand, and the institution of forced labor or corvée on the other) may not be equally apparent in the miniature water-work societies of the Pueblos. But nevertheless it can be shown that the same factors which led to certain social developments in the great irrigation complexes of the Old World have also operated in varying degree in the semi-desert regions of our Southwest.[83] Did they operate at all in the marginal lands of the Navaho?

Kimball and Provinse write:

> Simple observation reveals that there are many instances in which necessary activities are performed by persons drawn from several families.[84] . . . Cooperative community labor and membership exhibits itself most often in general problems relating to range use, water, subjugation and development of farm land, irrigation systems, and the presentation of a united front toward those who attempt to encroach upon the community rights.[85]

Today tribal warfare is more a legend than a threat, and the "united front" against the U. S. Government or any others "who attempt to encroach upon the community rights" may more properly be considered a matter of local solidarity than of technical cooperation. But such technical cooperation must have been required in former times to build the defensive walls and watch towers noted by contemporary observers[86] and present-day archaeologists.[87]

Of the cooperative activities in herding and farming there can be no question. Clyde Kluckhohn and Dorothea Leighton write:

> Some of the tasks . . . especially animal husbandry and agriculture, are carried out more often than not by a wider group of relatives than the simple biological family.[88] . . . Two or more different extended families, or one or more extended families linked with one or more independent biological families, may habitually pool their resources on some occasions—say, planting and harvesting or the giving of a major ceremonial. . . .[89]

Malcolm Collier who has made a special study of local organization in two Navaho communities notes that, at Klagetoh, where fields were cultivated before the "long walk" to Ft. Sumner, twelve complexes composed of a varying number of hogan-groups

> represent the basic cooperative units, the units which work together daily and almost invariably;[90] . . . There are also some occasions when a few of the people from one of these units work with people in another unit. This latter type of cooperation was never

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[83] Wittfogel and Goldfrank, 1943, pp. 19 ff. [84] Kimball and Provinse, 1943, p. 20.
[85] *Ibid.*, p. 23. [86] Farmer, p. 70, reference to letter of Ugarte y Loyola written in 1788.
[87] *Ibid.*, pp. 66 and 69; cf. also Keur, 1941 pp. 40 ff.
[88] Leighton and Kluckhohn, MS. [89] *Ibid.* [90] Collier, 1939.

as regular as the work within the above units but in the early days was more regular and frequent than it is now.[91]

For the recently established community at Navajo Mountain where the basic cooperative unit is the smaller hogan-group, she reports:

There is never any cooperation activity between two hogan-groups for herding, but there is a great deal of help in agricultural pursuits."[92]

W. W. Hill in his monograph on Navaho agriculture remarks:

Planting, especially corn planting, was usually accomplished by communal effort. When an individual was ready to plant he prepared an extra supply of food and sent out word that on a certain day he would plant. The people in the vicinity gathered in his field on the day named. Those who assisted usually included all the people in the community. No particular relatives were called. No pay was given these helpers, other than feeding them. However, the one whose field was planted was supposed to tender his services to his neighbors when their fields were planted.[93] . . . Communal planting is still being practised. My interpreter planted his corn and beans on June 12th. . . . He sent word to his neighbors who responded with fifteen teams and fifty men.[94]

In all probability the sixty Navaho who, in 1852, "entered a complaint against the Laguna Pueblo Indians for taking water needed by them to irrigate their lands" also represented the manpower of a "basic cooperative unit."

In her study of Navajo Mountain, Collier has observed that "the common fields . . . are large fields situated in Paiute Canyon. These are, in general, the oldest."[95] It seems reasonable to assume that at Klagetoh the older fields were also larger and more often "common," since cooperation there was greater in former times.

A point may also be made regarding the cooperative potentials in herding and irrigation agriculture. While cooperation in the former is frequently mentioned, it is important to note that as the animals grow in number, the original grazing land cannot support them indefinitely. When new pastures are sought, the herds are frequently divided,[96] and joint effort weakened. On the other hand, where agriculture depends on water-control, cooperation is not only necessary, but it increases with the increase of usable water and land.

In re-examining Kimball and Provinse's statement on cooperative community labor cited above, it becomes evident that this cooperative labor arises primarily from the problems concerned with intensive agriculture, or, as they have concretely expressed it, "problems relating to . . . water, subjugation and development of farm land, irrigation systems." Moreover, history and archaeology make it more than probable that the basic units of Navaho cooperation, which operate today in herding and ceremonial, and formerly

[91] *Ibid.* [92] Collier, 1940; see also 1939. [93] Hill, 1938, p. 28.
[94] *Ibid.* [95] Collier, 1939. [96] Hudson, 1938, pp. 21 ff.

also in defense, have developed out of the specific requirements of agriculture in a semi-arid environment.

After discussing Navaho political structure with twelve different individuals from ten different areas, Hill comments, "It is of more than passing interest that there was an almost complete unanimity of opinion by informants on this phase of Navaho culture."[97] In view of this, his data on Navaho leadership assume considerable weight.

"Community leadership," he writes, "was vested in one or more individuals whose duties involved direction of domestic affairs and warfare."[98] Under particular conditions of stress, these two areas might be temporarily merged, but Hill continues, "The Navaho recognized a distinct dichotomy in these two cultural phases and seldom did one man fill both offices."[99] Not only did one man seldom fill both offices, but service in the two fields, besides requiring different preparation, reflected different methods of choice and different attitudes toward tenure.

Anyone who knew one or more War Ways could be asked to lead a war party,[100] which might include as few as four or as many as two hundred men.[101] But complete command over the members was limited to the duration of the engagement.[102]

The selection of a local headman or peace *natani* depended upon more than ritual knowledge and the personal preferences of a frequently small group: "The people of a district met or were canvassed to determine the eligible candidate,"[103] and in most cases, "the choice was nearly unanimous."[104] Ritual installation followed and tenure "was assumed to be for life."[105]

The Franciscan Fathers in their Ethnologic Dictionary of the Navaho Language, write:

> The headman directs, for instance, in matters pertaining to agriculture, taking out ditches, or clearing and breaking new soil. He is arbiter in matters of dispute for the people of his district, whose interest he also represents at the councils (and with the agent).[106]

And Hill writes:

> The *natani* acted as general economic director and encouraged productive activities. He planned in advance the work for the community, set the time for planting, and superintended planting, cultivation, and harvesting. He instructed in the proper techniques of agriculture and pastoralism. Any communal work was under his supervision.[107]

While the headman's concern with arbitration and things pastoral must

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[97] Hill, 1940b, footnote, p. 23. [98] *Ibid.*, p. 24. [99] *Ibid.* [100] *Ibid.*
[101] Hill, 1936, p. 7. [102] *Ibid.* [103] Hill, 1940b, p. 25. [104] *Ibid.* [105] *Ibid.*
[106] Franciscan Fathers, 1910, p. 423. [107] Hill, 1940b, p. 27.

not be overlooked, it seems all but certain that it was Navaho agriculture, an agriculture depending largely on water-control and cooperative labor, which made some kind of stable and centralized community leadership necessary.

Ritual evidence makes the point even clearer: The local headman had to "control at least the Blessing Way Ceremony,"[108] which Father Berard describes as

> a rite whose legends, songs and prayers are chiefly concerned with the creation and placement of the earth and sky, sun and moon, sacred mountains and vegetation, the inner forms of these natural phenomena, the control of the he and the she rains, dark clouds and mist, the inner forms of the cardinal points and life phenomena that may be the harbingers of blessing and happiness.[109]

The inclusion of "vegetation" (not animals) among the great natural phenomena with which this rite is concerned is indeed revealing.

But even more so are the recorded rituals at installation. One informant claims that the newly chosen leader selected two perfect ears of corn, one white and one yellow. Carrying them to a pond, he deposited the white ear at the edge and then walking to the center of the pool, threw the yellow one in. This act he repeated with the white ear. "At the conclusion of his speech he recovered the two ears of corn and preserved them for ceremonial purposes."[110] Another informant insisted that "it was customary for the newly elected man to journey to the four sacred mountains and plant corn at each one."[111] Geography or a bad memory may account for these very different performances, but in one important respect they are strikingly similar: both assert the primary interest of the community leader in agriculture.

VARIATIONS IN THE STRENGTH OF LEADERSHIP

The adoption of agriculture and agricultural techniques from the Pueblos seems obvious from archaeological and linguistic evidence, and from Navaho legend as well. But the political organization that resulted, while exhibiting many Pueblo features, by no means mechanically imitated Pueblo institutions. Cooperative units and an organizing leadership, present in both cultures but different in each, manifestly reflected the specific needs of a similar subsistence economy in a more or less similar environment. Furthermore, the uniformity of opinion expressed by Hill's informants regarding the basic aspects of Navaho political structure should not obscure the fact that leadership in this inadequately watered and topographically varied Navaho region is sensitive to both time and place. The natural community, geographically determined and economically self-sufficient, is the political unit.[112] But some districts are iso-

[108] *Ibid.*, p. 25.
[109] Leighton and Leighton, 1944, p. 26, quoting Father Berard Haile, 1935.
[110] Hill, 1940b, p. 27. [111] *Ibid.* [112] *Ibid.*, p. 23.

lated; some are in close touch with other Indians, white traders, or government services of all kinds. Some have been inhabited for a long time; some have been settled for less than half a century. Some are, comparatively speaking, densely populated; some are sparsely peopled. Some are predominantly agricultural; some are predominantly pastoral.[113]

Collier writes:

> Leadership at Navajo Mountain was fairly amorphous and when it did exist seemed to fall on the older people simply because of their age and experience. . . . The people at Klagetoh have been conscious for many years of the existence of leaders in the country around them. They themselves have been under one leader for about thirty years.[114]

Klagetoh was a farming district before the Navaho were confined to Ft. Sumner. It lies in that portion of the Reservation where conditions are favorable to agriculture, particularly irrigation agriculture. Here the larger cooperative units operated and here a strong and stable leadership developed.

On the other hand, it was not more than fifty years ago, in the hey-day of pastoralism, that the first permanent Navaho settlement was established at Navajo Mountain. For thirty years thereafter the region served White-Man Killer and his family (no others migrated there) as winter quarters only. Under these circumstances agriculture must have been negligible and, in 1916, Gregory fails to mention it. In Paiute Canyon where the inhabitants of Navajo Mountain have their "oldest" fields, he merely notes the farming activities of a Paiute stockman.[115] In 1932, the U. S. Government built a dam in the vicinity.

Whatever the extent of their earlier agriculture, there can be no doubt that the new improvements markedly increased the agricultural productivity and the agricultural income of the Navajo Mountain residents, but, despite noteworthy cooperation in planting and harvesting between hogan-groups, leadership remained weak and amorphous, still responding, it would seem, to the strong pastoral tradition of the region. Whether time will change this picture is a matter of conjecture; but it is suggestive that concern with Navaho leadership is growing as Navaho land is being turned back from grazing to irrigation agriculture. Today, practical anthropologists with an understanding of native organization are urging government agencies to recognize again "those weighty substantial men"[116] in whom authority and control are vested —the local headmen, the community leaders.

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[113] For statistics on these last, see Soil Conservation Service, 1938.

[114] Collier, 1939a.

[115] Gregory, 1916, p. 105.

[116] Kimball and Provinse, 1943, p. 24.

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NOTES ON NAVAHO SUICIDE

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A PREVIOUS study of Navaho eschatology revealed a need for investigation of the Navaho's belief concerning the disposition of the spirits of suicides.[2] Data were gathered, therefore, in the Chaco Canyon, New Mexico, region, during the 1942 field session of the Chaco Canyon Research Station of the University of New Mexico. DS served as interpreter, and the following eight informants were employed[3]:

RA—male, *ca.* 48 yrs., clan 25, 24, 27, 26, 8,[4] (AT's "half bro."), M, NA, Chaco Canyon region.

AT—male, *ca.* 56 yrs., clan 25, 24, 27, 8, (mo's. fa. was Jemez), M, NA, S (Flint, Enemyway blackening), Chaco Canyon region.

FJ—male, 32 yrs., clan 32, 38, 37, 34 (CY's bro.), M, A, Chaco Canyon region.

CY—male, 47 yrs., clan 32, 34, 12, 35, 38, M, NA, Chaco Canyon region.

JA—male, 68 yrs., clan 29, (TW's bro.), M, NA, S (Navaho Wind, Upward-reaching, Blessing), Huerfano Mountain.

LU—male, 68 yrs., clan 9, 7, 10, 12, M, NA, S (Upward-reaching blackening), east of Blanco Trading Post (State Highway 55).

TW—female, *ca.* 75 yrs., clan 29, W, NA, Chaco Canyon.

DCW—female, *ca.* 67 yrs., clan 9, 7, 12, 10, M, NA, Chaco Canyon region.

DS—(interpreter) male, 50 yrs., clan 35, M, A, I, S (Gameways), Two Wells.

Descriptions of nine suicides and two unsuccessful attempts were obtained from these informants. Versions of one case (2; see table 1) were obtained from five different informants (FJ, CY, RA, TW, LU), including a son (FJ) and a nephew (CY) of the victim, and an elderly man (LU) who lives about twenty-five miles from the scene and who gave a curiously distorted account. Through the kindness of Drs. Clyde Kluckhohn, A. and D. Leighton, Gladys A. Reichard, W. W. Hill, and Walter Dyk, we are able to include data from their notes on cases 13 and 14; 15, 16 and 17; 18; and 29 and 30 respectively. Mrs. Elna Smith Melvin of Blair, Nebraska, very generously allowed us to employ data which she had gathered by questionnaire in 1934 from the records of the superintendents of the Navaho Agencies of that time (cases 19 to 26 inclusive). Cases 27 and 28 are from previously published reports[5] and are the only concrete instances which we have been able to find in the literature. Case 31 is from the unpublished field notes of the late William Morgan. These thirty-three (in our subsequent analysis suicides and unsuccessful attempts are not differentiated) are presented schematically in the following table. In contrast

[1] Boston University School of Medicine, Boston, Mass.

[2] See Wyman, Hill, and Osanai, p. 40, footnote 75. (See Bibliography at end of article.)

[3] See *ibid.*, pp. 9–10, for the conventions used to describe the items of their status.

[4] Multiple clan numbers indicate that the informant was listing the clans of his clan-group: *cf.* Reichard, 1928, p. 36 ff.; Carr, Spencer, and Woolley. They are in the order given by the informant, so the first is probably his own clan affiliation.

[5] Case 27—Bourke, p. 85; case 28—van Valkenburgh, p. 54.

Case No.	Informant	Authority		Date	Locality	Victim		Methods			Causes-motives			
											Responses: love-motives		Avoidances	
		Intimate knowledge	Hearsay			Sex	Age	Shooting	Hanging	Jumping from cliff	Grief over death of relative	Marital quarrels and jealousy	Consequences of crime	Incurable illness
1	AT	X		*ca.* 1900	Chaco Canyon	M	AMC	X			X			
2	*	X		*ca.* 1920	Chaco Canyon	M	AMC	X				X	M	
3*	FJ	X		*ca.* 1920	Chaco Canyon	F	AMC	X			X			
4	AT		X	*ca.* 1916	?	M	AMC	X			X			
5	FJ			*ca.* 1939	?	M	A	X						X
6*	JA	X		*ca.* 1920?	Huerfano Mt.	F	A							X
7	JA				?	M	AM				X			
8	JA				?	M	AM					X		
9	JA		X	*ca.* 1939	?	M	A		X					
10	DCW		X	1942	Shiprock	F	AM	X				X		
11	*	X		1942	Fruitland	M	AMC	X			X		M	
12	DCW		X	1942	Tohatchi	M	A						D	
13	*	X		*ca.* 1914	Ramah	M	AM		X		X		M	
14	*	X		*ca.* 1920	Ramah	M	A	X					S	
15*	*	X		*ca.* 1910?	Two Wells	M	AM		X			X?	M	X?
16*	*	X		*ca.* 1930	Shiprock	M	AMC	X			X			
17	*		X	*ca.* 1930	Shiprock	M	A	X						X
18	*		X	?	Fort Defiance	M	AM			X		X	M	
19	*			1929–34	Rehoboth	M	AM	X				X	M	
20	*			*ca.* 1895	Crownpoint (?)	M	A	X						
21	*			*ca.* 1910	Thoreau	M	A		X				R	
22	*		X	1929	Tuba City	M	A		X					
23	*			1929–34	Fort Defiance	M	AM	X				X	M	
24	*			1929–34	Fort Defiance	M	A							
25	*			1927	?	M	A							
26	*			1918	?	M	A		X				M	X
27	*		X	*ca.* 1878	Awatobi	M	AM			X		X		
28	*		X	"yrs. ago"	Frazier's	F	A			X			I	
29	*	X		*ca.* 1901	Lukachukai	M	AMC	X				X	M	
30	*	X		1932	Lukachukai	M	AMC			X				X
31	*	X		*ca.* 1930?	Coolidge (?)	M	A		X				RM	
32	*	X		1944	Fort Defiance	M	A	X						
33*	*	X		1944	Wingate	M	AM	X				X		

* *Case Nos.* 3, 6, 15, 16, 33—unsuccessful attempts (17 may have been murder rather than suicide). *Informants* for 2—FJ, CY, RA, TW, LU; 11—Farmington Times Hustler, Sept. 18, 1942; 13—9 or 10 Navahos and Mrs. Masters (Zuni); 14—3 Navahos; 15—victim's nephew; 16, 17—a trader at Shiprock, N.M.; 18—W. W. Hill (from memory); 19, 20, 21—Supt., Eastern Navaho Agency; 22—Fld. Physician, Western Navaho Agency; 23, 24—Supt., Southern Navaho Agency; 25—Physician formerly on Navaho Res.; 26—Supt., formerly on Navaho Res.; 27, 28—see footnote 5; 29—clan bro. of victim's 2nd wife; 30—friend of victim; 31—trader at Coolidge, N.M.; 32, 33—Gallup Gazette, July 20 and Sept. 28, 1944. *Dates* are mostly approximate only. *Localities* are in New Mexico, except Fort Defiance, Tuba City, Awatobi, Frazier's, and Lukachukai which are in Arizona, and mostly designate regions. *Sex:* M—male, F—female; case 3—mother of victim in case 2, said to have attempted suicide in frenzy of grief over death of her son. *Age:* A—adult, M—known to be married, C—known to have children; 17, 31—"a boy"; 32—discharged veteran, 23 yrs. *Methods:* 6—stabbing; 25—poison. *Causes:* 20—drunkenness; 22—"social-sex matter"; 25—quarrel with relatives over settlement of mother's estate; 32—said to have quarreled with a young woman. *Consequences of crime:* M—murder, S—cattle stealing, D—suicide to avoid military draft, R—rape, I—clan incest.

to Fenton's Iroquois data,[6] there were no cases of child suicide, indeed the majority of victims being of middle age or above (although five of the eight from Mrs. Smith Melvin's data were in their middle or late twenties—cases 19, 22, 23, 24, 25), and male suicides predominated (29 of 33 cases).

Navaho informants say that suicide is infrequent. Lack of data for the actual incidence per unit of time and population, either for the Navaho as a whole or for any given region, prevents comparison with other societies. Judging however from the fact that eight informants, one of whom was 75 years of age or over and three around 68, reported only three suicides (or attempts) in the Chaco Canyon region within their memory (and two others which might have been in the area), and since two of the three were in connection with the same incident (suicide and attempt by the mother of the victim—2, 3), we may venture a guess that the frequency of suicide among the Navaho is not conspicuously different than in our own society. Moreover, individual informants seldom knew of more than two cases from their own or any other region. Our informants' opinions as to the comparative frequency of suicide in recent years and in "the old days" varied so that no conclusion could be reached (formerly less frequent—3, more frequent—1, same—1, undecided—3).

The general infrequency of suicide among the Navaho and its rarity or comparative absence in former times is attested, however, by the statements and the concrete data from the records of the superintendents of four of the former agencies (see table; no recollection among older Indians of any suicide nor any attempts—Supt., Leupp Agency, Arizona) collected by Mrs. Smith Melvin. Only eight cases were reported, six from three of the agencies and two by former officials on the reservation, and of these all but one occurred between 1910 and 1934. No records of suicide appear in the field notes of Malcolm Carr Collier from extensive studies in the Navaho Mountain and Klagetoh, Arizona, regions, although it is true that she was not making specific inquiries about it. Dr. Hill tells us that his informants said that suicide was formerly unknown and it was not until recent years that sporadic cases began to occur. Moreover we find in Hrdlička's monograph of 1908 the following: "Suicides occur among most of the tribes visited, but on the whole they are rare, especially among the women."[7] Only four of our thirty-three victims were women. Finally, reference to the table shows that only two of the twenty-nine dated cases were in the late nineteenth century and that all but two of the others occurred since about 1910, fourteen of these since about 1930. This distribution is congruent with the fact that the last decade and a half has been a period of greatly increased group tensions.

The methods employed for committing suicide appear to be dictated by convenience. In 29 cases for which the method was specified, shooting was

[6] Fenton, p. 123. [7] Hrdlička, p. 171.

used in 16 and hanging in 7. It is curious that a cutting instrument was employed in only one (6: unsuccessful attempt—woman), although three informants (LU, CY, DCW) mentioned the knife as one of the suicide implements carried by the victim in the afterworld.[8] Upon direct questioning, informants averred that shooting is the commonest method (LU, AT), and that use of a knife is infrequent (JA). The scarcity of considerable bodies of water in the Navaho country could easily explain the absence of drownings. Similar reasoning, however, would not apply to the infrequency of leaping from heights ("lover's leap"), but two informants mentioned jumping from a cliff as an obsolete practice of the senile (JA—hearsay only) and in speaking of the afterworld (TW), one case was reported to Dr. Hill (18), one to Dr. Dyk (30), and two cases have been published. One in Arizona about 1878 resulted from marital jealousy and quarreling[9] and the other "years ago" was a "story" of a girl who had committed clan incest (a real "lover's leap").[10] Perhaps this method is an obsolete or obsolescent action pattern.[11] The most striking negative characteristic is the comparative absence of poisoning (only one case—25), an action pattern which was the most frequent method, especially among women, in the Iroquois suicide pattern assemblage,[12] and which was an accepted method in several tribes of northeastern California.[13] LU never heard of poisoning and remarked that the Navaho "couldn't get hold of (poison) any place; white people watch pretty close for that; whites don't want to have that around Indians." Nevertheless, Jimson weed (*Datura spp.*) is quite common in many parts of the Navaho country, and most Navahos are quite familiar with its poisonous properties. *Cicuta* (water-hemlock), which was the "suicide root" of the Iroquois and of northeastern California, likewise occurs and is known to Navaho herbalists at least (*C. occidentalis* Green—*šildcoh*—"my thumb"), but its distribution is limited to moist places and is such that it would not be readily available to the average Navaho. Avoidance of the use of *Datura* remains to be explained however. Since this plant is employed in medicine,[14] in certain ceremonials, and especially in a form of witchcraft (frenzy witchcraft)[15], perhaps this avoidance arises from a configuration which might be expressed as "avoidance of the misuse of ceremonial appurtenances." This would be especially cogent if the appurtenances were likewise misused by "witches." One often hears concerning *Datura*, "they (the Navaho) are kind of afraid of it," and this refers to mere handling as well as to more intimate contact. The only victim of poisoning reported (case 25) employed poison which had just been distributed by the Superintendent to kill coyotes.

Some ingenuity occasionally appears in the application of the two more

[8] See Wyman, Hill, and Osanai, p. 39. [9] Bourke. [10] van Valkenburgh.

[11] The victim of case 17 was said to have first threatened to throw himself from a cliff but was dissuaded by his younger sister. [12] Fenton. [13] Voegelin.

[14] See Wyman and Harris, p. 59. [15] See Kluckhohn, p. 23.

common methods, as when a victim tied one end of a string to the trigger of his rifle and the other end to his toe (case 2), or when another tied a rope to the crossed beams projecting from the edge of his hogan roof, measured the rope, adjusted the noose, and jumped down through the smoke-hole (case 15—but the rope was too long).

Of thirteen cases having pertinent data, in eleven the suicide was committed at or near the victim's home (in the hogan [6, 14, 15, 29, 32], a tent [11], or a sweat-house [5]; on a nearby hill [13], mesa [17], or cliff [28, 31]; away from home [2, 33]). Somewhat elaborate preliminaries were described for only one case (2). Here the victim donned his best clothes and jewelry, put new saddle, bridle and blanket on one of his horses, rode some distance from home, shot the horse through the head, and then shot himself, thus attending to the usual mortuary practices for himself.[16]

In five cases grief over the death of close relatives (wife or children) was given as the cause of suicide, and in two others (11, 13) it may have been the initiating factor in the chain of events leading to the killing of suspected witches and suicide. The informant for case 16 told Dr. Reichard that he knew of two other cases where grief at the loss of a grown child caused suicide, remarking that "the Navaho are most dangerous when grief-stricken." Marital troubles were present in nine cases, jealousy and quarreling leading directly to suicide in four of these (8, 10, 27, 33), and family quarrels and jealousy being the initiating factors followed by murder and suicide in five others (2, 18, 19, 23, 29). In one other case (15) the murder of a younger wife preceded a suicide attempt, although chronic illness and possible mental aberration may have been involved here instead of marital trouble. In "Son of Old Man Hat" (Walter Dyk, 1938, p. 48) Left Handed's mother warns him that "It's pretty dangerous to have a wife or a husband. Some men, when they have wives, may kill their wives or may get killed by them, and some commit suicide." In ten cases avoidance of capture and punishment by whites appears to have been the motive for suicide, following murder in eight cases (of suspected witches in two of these—11, 13), cattle stealing in one (14), and rape of a little girl in another (21). In four cases (5, 6, 17, 30) desperation because of incurable illness was the cause of suicide, in another (15) it may have played some part in a series of events culminating in a suicide attempt, and the victim of case 26 was said to have been "crazed during a flu epidemic after killing entire family with an axe." In one case (12) avoidance of the military draft was given as the motive for suicide, in one it was said to be a "social-sex matter" (22), in one the victim was said to have quarreled with a young woman (32), one resulted from a drunken spree (20), another from a "difference with relatives over settlement of mother's estate" (25), and in two (9, 24) no cause or motive was known to the informant.

[16] See Wyman, Hill, and Osanai, pp. 22, 23.

The data are too meagre for generalization, but at present no one cause appears predominant for women: grief, jealous quarreling, illness, and clan incest accounting severally for the four cases. The reasons for suicide ventured by informants when asked to generalize corresponded closely with those found for the actual cases.[17] Suicide for economic reasons did not appear in any case history and had not been heard of by informants. Hrdlička in 1908 stated that "Among the Navaho rare instances of suicide occur from destitution, despondency, or drink." We have confirmed the second and third of these (19 and 20 occurred during "drunken sprees"), but not the first.

To summarize, the principal motives for our thirty-three cases of suicide were response to love-motives (grief over death of relatives, marital jealousy and quarreling) and avoidance of the consequences of crime or illness.[18] The distribution was roughly equal for the two motive groups and for the two causes for each. These motives and causes are not unlike those existing in other cultures, Indian and white, but the whole group differs in the absence of certain motives common in other cultures, e.g., the economic causes of suicide in "white" society, and the revenge for mistreatment (and recognition-seeking) motives characteristic of the Iroquois.[19]

The behavioral action pattern of the sequence *murder-suicide* (or *crime-suicide*) and its accompanying avoidance motive may be a signal of a configuration something like "avoidance of trouble with white legal processes." Definition of the pattern as major or minor, conformant or deviant, requires further study of its incidence and comparison with that of murder alone in the culture as a whole. Statements such as "sometimes person can do wrong things which will be very bad for him, then he have to kill himself" (AT) hint at its being conformant with the ideal pattern, but the consensus of disapproval of suicide and certain other statements ("some other people get into trouble, but they don't kill self"—LU) militate against its conformity.

We were not able to learn of an abstract term for suicide. Navaho employs only reflexive forms of the verb "to kill,"[20] e.g. *ʔáái·łɣé*—he kills himself, *ʔáái·sɣɬ*—he killed himself, etc. Father Berard Haile very kindly told us of these terms and added "Navaho prefers the verb 'to suicide' rather than a noun 'suicide,' perhaps because a personal action is involved in suiciding.

[17] The only additional cause given was senility, an obsolete pattern according to JA (*v.s.*). Allied to this may be the reported belief that an aged Navaho can end his life by wishing to die; see Newcomb, p. 79.

[18] The "lover's leap" reported by van Valkenburgh (case 28) may have been a love-motive response, but according to the story it appeared more as an avoidance of censure for incest.

[19] See Fenton, pp. 124–128. Navaho motives are strikingly similar, however, to those given for the Chiricahua Apache by Opler, pp. 250, 409, 472. For data from the Pueblo Indians, where "suicide is almost unheard of," see Parsons, p. 75.

[20] As in the Iroquoian languages; see Fenton, p. 85; "While this is typical of Iroquoian languages which generally have few abstract terms, it does show that the act was not frequent enough to cause the progressive reduction of the descriptive verb to an abstract concept."

Thirds may also function in an impersonal sense so that *ʔádi·lγé* also means 'there is suiciding, or a suicide,' and so on for the perfective and future, which is pretty close to our abstract noun 'a suicide'." Dr. Clyde Kluckhohn tells us that in the Ramah region the third person singular perfective (*ʔádi·sγɬ*) is used as a noun (colloquially) to mean "the suicide," i.e., "the man who commited suicide."

Father Berard also told us that "There is another word that approximates suicide, namely *ʔádąhnaxastą́·go*, 'when he or they put time conditions upon themselves' or he exposes himself and takes chances on life. A person may feel himself cornered and is ready to defend himself to the last regardless of consequences. The inference is that such a person realizes that his action is suicidal but proceeds anyway. To induce another to such action is not approved."

None of our informants had ever heard of an origin myth for suicide, nor any mention of it in any of the myths known to them. RA related the story of the first death from the emergence legend,[21] and added "Many years after that, people started to make story people who kill self wouldn't go someplace same but separate from other group down there." One of Father Berard's informants rationalized the descent of a man into Emergence Place to join the spirit of his former wife, in a version of the first death, as accounting for suicides today, but other informants said that suicides were comparatively rare.[22] We have been able to find only one reference to suicide in the recorded legends which we have seen, and here there is some question as to whether it was suicide or accidental death, different versions varying.[23] We may assume (pending future discovery) that suicide was not sanctioned in Navaho mythology.

If one were to judge alone from the statements in English of the interpreter the conclusion would be that our Navaho informants had a rather casual attitude towards suicide. A victim is merely an unfortunate person who could have lived longer if he had done the right thing. The act is not condoned, neither is it greatly condemned. The usual expression was one of mild sorrow for the victim, and mild disapproval of the practice. For instance; "People don't like it—it's a great mistake. Would be better for person not to kill himself. Especially young people. If they live like they should then they would die someday" (AT). "Kind of feel sorry about it. People who do that they bring it up among themself" (DCW). "The people think it pretty bad. But they can't help it, after it happens. But feel sorry for person. Not very good thing to do" (JA). "Bad thing to do. They don't try to take care of self, wife, or family like the others. They could live longer if they did. They supposed to

[21] See Wyman, Hill, and Osanai, p. 36. [22] Haile, p. 412.

[23] The suicides by drowning during the separation of the sexes in the uppermost underworld, in the Emergence Legend: Matthews, p. 72; Goddard, p. 129. This episode would seem neither to account for nor to sanction the practice of suicide by the Navaho.

live long just like others. Like way they do they just cut their life off" (RA). "If he didn't do that he could live more" (CY). "Pretty bad. People says they just wonder why people have to kill themselves like that. They have to die anyway someday, might just as well not do it" (TW). "Not very good thing. That's all they can do, feel sorry for man who kill self" (LU). Navahos are sometimes prone to understatement,[24] however, especially when speaking in English. Our conclusion would be then, that suicide is condemned (although perhaps not violently), mainly because one should not shorten one's life and thus escape the responsibilities of life, e.g., caring for one's family, but after the act considerable sorrow is felt for the victim and for his family.

We obtained ample confirmation of the idea pattern that the spirit of a suicide must continually carry the lethal implement in the afterworld[25] (gun, knife, stick, rock—DCW, CY, LU), and that he is excluded from association there with the spirits of those who have died otherwise.[26] Six of eight informants expressed this belief and explained it somewhat as follows. "People who kill themself don't go where other dead people go. In crowd people scared of him, try to push him off. No friend down there any more after kill himself. They carry gun, knife, etc., so those people down there afraid of visitor. Afraid they kill people down there" (DCW). "They go same place where all dead go but they don't go into that crowd, they put them off, they afraid to have them with other people. They know these people who kill themselves they bad already so they afraid of those kind of people. They rather have them go separate from the others. All go same country where they can see each other but is different place" (RA). "They go different place. Have another place just from there. People afraid of him when he gets down there, have to put him off not to get in crowd. Still got with him whatever kill self with. Carry all time; want to kill man all time, that's why" (LU). Only TW said that the spirit of a suicide might be earthbound ("where a man kill himself he have to be right there where he kill himself, no other place. Stays there always.") but her further discussion of ghosts ("don't know whether ghosts of suicides do that"—i.e., behave like earth people in the afterworld as do the spirits of others) indicated uncertainty as to her beliefs. The other two informants (FJ, CY) said that the spirits of suicides go to the same place as those of others, but they did not discuss the matter of association with the other spirits, nor did they say anything contrary to the idea of exclusion. Indeed CY said "after kill self they go on where other people goes and take their gun who kill self with (or knife) and people say they are danger after they kill themselves."

Four informants (RA, CY, FJ, JA) stated that the usual burial practices

[24] Cf. Reichard, 1934. [25] Wyman, Hill, and Osanai, p. 39.

[26] *Ibid.*, pp. 39–40. This idea pattern should be added, therefore, to the summary of the assemblage of behavioral patterns of Navaho eschatology, possibly as a major pattern pending further study. See footnote 75, p. 40, and 5 d, p. 46.

are accorded to a suicide (dressing the deceased in his best clothes and jewelry, killing saddle horses at the grave, etc.) the same as to anyone else. These preparations for the afterworld and precautions against the return of the ghost[27] likewise betoken the beliefs that the destination of the spirit of a suicide and the behavior of his ghost (*v.i.*) are similar to those of any other dead person. Again these ideas are not concerned in the question of whether or not a suicide's spirit is allowed to mingle with the others in the afterworld. In summary, the idea pattern is that the spirit of a suicide goes to the afterworld and can return thence as a ghost (*v.i.*), but while there it may not live with nor in the same place as the spirits of other mortals (because they are afraid of it).

This pattern is similar to that of the Hidatsa ("self-murderer will live isolated in the future life, but will not be less well treated"[28]) but differs from the Iroquoian belief that suicides are earth-bound, excluded from the land of the dead.[29]

Most informants thought that the ghost of a suicide is of the same nature as that of anyone else.[30] Three (JA, CY, DCW) definitely stated this as true, two (AT, TW) said that they did not know of any difference, and two (RA, FJ) did not express any opinion but their discussion of kindred matters indicated that they knew of no ideas to the contrary. One woman (DCW) did say that ghosts of suicides might "come around more often; don't know just how often." Only one informant (LU) said that the ghost of a suicide is more dangerous than that of someone else. "They more afraid about people who kill self. They mad all time, got mad when start to kill self. When come back already mad." In spite of the reasonableness of this idea it seems to be a minor deviant pattern.

SUMMARY AND CONCLUSIONS

The pattern assemblage of suicide among the Navaho (at least in the area studied) is limited with regard to content as well as to occurrence. Victims are mostly in the upper age levels and are predominantly male. The act is usually committed at home. Methods are mainly limited to shooting and secondarily to hanging (leaping from a cliff possibly being an obsolescent pattern). The principal causes and motives for suicide are response to love-motives (grief over death of relatives, marital jealousy and quarreling) and avoidance of the consequences of crime or illness, numerous other motives present in other cultures being absent. A well crystallized abstract term for suicide does not exist and there is no origin legend for the practice nor any sanction of it in Navaho mythology. These limitations of the pattern assemblage may partly account for the attitude of not too violent disapproval of suicide and sorrow for the victim. Superficially they indicate that suicide is

[27] See *ibid.*, p. 23. [28] See Steinmetz. [29] Fenton, pp. 89, 131 ff.
[30] For details of the nature of ghosts see Wyman, Hill, and Osanai.

a relatively unimportant factor in Navaho culture, but Kluckhohn warns us most pertinently that " . . . discriminations on the basis of extensiveness of cultural knowledge or of time devoted to an activity are dangerous," since "cultural 'importance' has an intensive, a qualitative, dimension as well." For the individual, attempted suicide is eminently adjustive (successful suicide ultimately so, in the sense of the hoary quip *re* the suicide, "I hope that will be a lesson to him"), but it may be fairly said that for Navaho society at large if suicide were as important an autistic reaction to deprivation as is, for instance, witchcraft, we might expect a less limited pattern assemblage, with mythological sanctions, terminology, techniques, distribution within certain time and space categories, etc., comparable to, let us say, those of witchcraft. To paraphrase Kluckhohn,[31] suicide, then, takes its place as but one of a number of phenomena where the interaction between the Navaho organism and the changes in his environment may be observed and studied.

In the pattern assemblage of eschatology there are (major-?) idea patterns that the spirit of a suicide goes to the afterworld and can return as a ghost, whose nature is the same as that of other ghosts, but while there it must continually carry the suicide implement and is not allowed to live with nor in the same place as the spirits of other people, because they fear it.

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[31] Kluckhohn, p. 71.

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THE CASE FOR BLACKFOOT POTTERY

By JOHN C. EWERS

ONE of the most perplexing problems to students of Blackfoot Indian material culture in the last half century has been this: Did the Blackfoot make pottery?

In 1892, George Bird Grinnell, a serious student of Blackfoot life, answered this question in a reserved negative. "It is doubtful if the Blackfeet ever made any pottery."[1] At the time he made this statement there was no evidence to the contrary in print. The American artist, George Catlin, had met some of the Blackfoot at Fort Union in 1832. Prince Maximilian, the careful German scientist-explorer, had spent a month in the heart of the Blackfoot country in the summer of 1833. The Canadian artist, Paul Kane, and the missionary priest, Father De Smet, had visited the Blackfoot on more than one occasion in the '40's. All had published their observations on Blackfoot culture in considerable and valuable detail. Yet the subject of pottery is not mentioned in any of their writings about these Indians.

Within the past half century a number of important descriptions of the Blackfoot, by fur traders who knew them well in the late years of the eighteenth and early decades of the nineteenth Century, have been published. The majority of the best of these accounts, i.e., those of Alexander Henry the younger (published in 1897), of Charles Larpenteur (1898), of David Thompson (1916), and of Duncan McGillivray (1929), are silent also on the subject of Blackfoot pottery.

Nevertheless, considerable new evidence in the case of Blackfoot pottery has been brought to light during the past half-century, in the form of two pertinent manuscripts, one from the eighteenth century and one from the nineteenth, which were not published until the present century; in the form of testimony by Blackfoot Indians living in the present century regarding traditions of pottery making among their people; and in the form of new information on the practice of the potter's craft by neighboring tribes. All of this evidence is fragmentary. Much of it is circumstantial, susceptible to more than one interpretation. Yet all of this evidence serves to infuse new life into the case, and to make it worthy of reconsideration at this time. Let us follow the new developments since the time of Grinnell's 1892 statement.

The silence of the early writers on the subject of Blackfoot pottery seemed to some students to have been broken when, in 1908, the *Journal of Mathew Cocking, from York Factory to the Blackfeet Country, 1772–1773*, was published. In his Journal, Cocking stated that, on December 1, 1772, he came to twenty-eight of "Archithinue Natives" at a buffalo pound (probably somewhere between the Eagle Hills and the present Alberta-Saskatchewan border).

[1] Grinnell, 1892, p. 202.

He remained with these Indians for several days and wrote a brief description of their camp life, which mentions their manufacture and use of pottery:

> Their Victuals are dressed in earthen pots, of their own Manufacturing; much in the same form as Newcastle pots, but without the feet.[2]

Two months earlier (October 7, 1772) while en route westward, Cocking had noted also:

> I found in an old tent-place belonging to the Archithinue Natives part of an earthen vessel, in which they dress their victuals; It appeared to have been in the form of an earthen pan.[3]

Cocking went to some pains to identify those Indians he met at the buffalo pound. The italics are ours:

> *This tribe* is named Powestic-Athinuewuck (i.e.) Waterfall Indians. *There are 4 Tribes, or Nations, more,* which are all Equestrian Indians, Viz., Mithco-Athinuwuck or Bloody Indians, Kostitow-Wathesitock or Blackfooted Indians, Pegonow or Muddy-water Indians & Sassewuck or Woody Country Indians.[4]

Lawrence J. Burpee, editor of Cocking's *Journal*, was inclined to ignore the tribal distinctions made by Cocking in his identification of the Indians he met. Burpee speaks of them only as "Blackfeet" in his introductory commentary, and in an earlier introduction to the *Journal* of Anthony Hendry he points to Cocking's statements as conclusive refutation of Grinnell's position regarding "Blackfeet" pottery.[5] A number of more recent writers on the Blackfoot have accepted Burpee's interpretation of this point. We cannot follow them. Cocking seems to make it perfectly clear that the Indians he met at the buffalo pound were neither Bloods, Blackfoot, nor Piegans. The native term used to identify those Indians differs little from the name still given the Gros Ventre by the Cree Indians of the Rocky Boy's Reservation.[6] The Gros Ventre were frequently referred to in the literature of the late eighteenth and early nineteenth century fur trade of the northwest as "Fall Indians." Although we cannot accept Cocking's descriptions as direct proof of the existence of Blackfoot pottery in the 1770's, his comments are not without considerable value. It is probable that Gros Ventre and Blackfoot material culture were very similar at that time. Since the Gros Ventre made pottery, it is highly probable that the Blackfoot did also.

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[2] Burpee, 1908, p. 111. [3] *Idem.*, p. 108.

[4] *Idem.*, pp. 110–111. [5] Burpee, 1909, p. 317.

[6] Recently the writer, in company with J. Willard Schultz, asked a small group of Cree Indians from Rocky Boy's Reservation, who were visiting the Museum of the Plains Indian, their names for the Gros Ventre, Piegan, Blood, North Blackfoot and Sarsi. Mr. Schultz, whose familiarity with the languages of the Northwestern Plains dates back more than a half century, was convinced that Cocking had identified these tribes correctly by their Cree names, as soon as he heard the terms given by our informants.

In the course of his field work among the Blackfoot tribes in the first decade of the present century, Clark Wissler collected traditions of Blackfoot pottery-making from two informants. At the time of their publication (1910), Wissler seemed to have no knowledge of Cocking's *Journal*, but he was thoroughly familiar with Grinnell's viewpoint. Wissler recorded his findings cautiously:

It is not certain that the Blackfoot ever made pottery, though some individuals claim such information to have been handed down to the present generation. An old woman had heard that cooking pots were once made of pulverized rock and some sticky material. She never heard of pots hollowed out of stone. A man had heard that pots were made a long time ago. They were fashioned of mud and sand. A bag of rawhide was filled with sand, greased on the outside and the pot shaped over it. The sand was then poured out and the bag withdrawn. The pot was filled with fat and hung over the fire to harden. When finished, it was tested by boiling water in it. Such pots grew gradually harder with use. They were supported by a rawhide cord passing around the rim. The cord had to be changed often. He also heard that pipes were made of clay and hardened by holding over the fire. During this operation they were always kept rubbed with fat."[7]

In a footnote to this statement Wissler referred to the mention of pottery in the myth of The Twin Brothers, which he and D. C. Duvall had collected:

Now at this time, the people cooked in pots of clay. These were shaped out of mud by the hands, and put in the sun to dry; then the kettle was rubbed all over with fat inside and out, and placed in the fire. When it was red hot, it was taken out, and allowed to cool. Such a pot was good for boiling.[8]

Then Wissler concluded:

Aside from these narratives, there is no evidence that pottery was made by the Blackfoot. That these statements may represent intrusive traditions is suggested by the seeming parallels among the Gros Ventre and certain striking agreements with processes employed by the Mandan and other village Indians.[9]

Wissler stated the case for the nomadic Plains tribes in general:

So far, the use of pottery vessels has not been fully proven for the Assiniboine, Gros Ventre, Sarcee, Blackfoot, Crow, Arapaho, Kiowa, Comanche and Cheyenne. Some of these have traditions of pottery, but definite statements by explorers are wanting, leaving the case as tentative."[10]

Wissler seems first to have become acquainted with Cocking's *Journal* through a secondary source, Agnes C. Laut's *The Conquest of the Great Northwest*. Miss Laut had been no more critical than Burpee in designating the

[7] Wissler, 1910, p. 26. [8] Wissler and Duvall, 1908, p. 43.

[9] Wissler, 1910, p. 26. The Gros Ventre reference here is not to Cocking but to a brief statement in Kroeber, 1908, p. 150: "Pottery is declared to have been made formerly of clay mixed with crushed rock." [10] *Idem.*, p. 45.

Indians met by Cocking at the buffalo pound as "Blackfeet." Wissler recognized in Laut's interpretation of Cocking's statement the definite mention by an early explorer that he had been looking for to confirm the traditional evidence he had collected on Blackfoot pottery making. In an often-overlooked appendix to his *Ceremonial Bundles of the Blackfoot Indians* (1912), Wissler cited Laut's reference to Cocking's mention of Blackfoot use of earthen pots for cooking utensils, and proceeded to abandon his doubts in the matter of Blackfoot pottery:

> Now all our traditional information from the Blackfoot, the information we doubted, seems to indicate that in the manufacture of pottery they employed a method of no firing, that is, the vessel was shaped, dried, and then rubbed with fat, after which it was put on the fire and used. Quite recently, Mr. Skinner secured data from the Menomini of the same tenor, but still more definite, leaving little room for doubt that we have here a rather widely distributed type of no-fired pottery.[11]

This was the status of the case for the next decade. The argument for Blackfoot pottery rested upon the traditional evidence of two informants, a statement in a myth, and the reference to pottery in Cocking's *Journal*, which seems to have been misinterpreted by Burpee and Laut, and through Laut, Wissler, as a reference to Blackfoot pottery.

New and very important evidence was introduced in 1923, when the manuscript of the soldier-historian, Lieut. James H. Bradley, written a half century earlier, was published by the Montana Historical Society. In the mid-1870's Bradley wrote of Blackfoot pottery:

> They made in early times a sort of rude kettle of moistened clay, shaped with the hands, dried in the sun and then burned in the fire. These kettles ordinarily held about two (2) gallons and were of cylindrical shape and usually of greater breadth than depth. They were replaced by vessels of tin, brass, copper and iron as fast as they were able to buy them of the traders and they are no longer manufactured. It is doubtful whether a single specimen has been preserved to the present day.[12]

Lieut. Bradley was the most scholarly historian of the territorial period in Montana. He was stationed, for five years prior to his death in 1877, at Fort Shaw and Fort Benton in the Blackfoot Country. He was an indefatigable student of Blackfoot history and culture, who numbered among his informants intelligent fur traders, priests and Government officials, some of whom had known the Blackfoot since the early 1830's. It is probable that one or more of Bradley's informants had seen Blackfoot pottery made. But unfortunately, Bradley did not cite his source of information.

Bradley's evidence is important in spite of its brevity. It mentions a method of pottery manufacture, the size, shape and use of the vessels, and indicates that pottery making had become a lost art among the Blackfoot

[11] Wissler, 1912, p. 285. [12] Bradley, 1923, p. 257.

before the 1870's, due to the general substitution of metal trade kettles for clay cooking vessels. This would seem to explain why no twentieth century Indian informant claims to have seen Blackfoot pottery made, and why most modern informants have no knowledge of Blackfoot pottery whatever.

That is the extent of the published information on Blackfoot pottery. However, considerable unpublished data have been collected in the field during the past decade bearing on the subject. In view of the poverty of the published testimony it would be well to cite this new evidence in full.

In the summer of 1935, Kenneth Kidd questioned six North Blackfoot informants about the former use of pottery by that tribe. Five of them declared that they had heard or knew of its manufacture by their people. Kidd obtained brief descriptions of two distinct methods of pottery manufacture said to have been employed by the North Blackfoot.

> According to some informants the pots were made of a white sticky clay, found in rocky places, which was moulded into shape and stood beside the fire to dry. . . . Such pots were used for boiling.
>
> The other tradition relates that a bag of the required size was made from buffalo skin. Sand (evidently clay and sand) mixed with water was plastered on the inner surface of the bag and allowed to dry in the sun. The skin cover was then removed. Sometimes a handle made from the neck-gristle of a bison was attached through holes made in the rim.[13]

Pretty Young Man, who related this second tradition, was a middle-aged man who had a keen interest in the history and culture of his people. Kidd regarded him as an intelligent and an honest informant.[14]

During the past three years (1941–44) we have questioned a considerable number of older Piegan and a few Blood Indians regarding the culinary utensils formerly used by their people. The majority of them had no knowledge of Blackfoot pottery. However, four of them gave fragmentary descriptions of the native pottery which are revealing.

Weasel Tail, a Blood Indian in his middle eighties, recalled that when he was a young man, Victory All Over Woman, an aged Blood, reputed to have been over one hundred years of age at the time, told him how the people of their tribe made pottery in her youth. That must have been no later than the beginning of the nineteenth century.

> She said that to make a cooking pot they first dug a hole in the ground, making the sides as round and smooth as possible. Then they took a flat stone and placed it in the bottom of the hole. Then they lined the hole with a thick layer of clay. Next they took a mixture of a certain red rock which had been broken into small pieces with a stone hammer, heated and again pounded until the rock was as fine as flour, and river sand or sand rock pounded fine, mixed with water into a dough. After the clay lining was

[13] Kidd, 1937, pp. 114–115. The author has kindly granted permission to cite this reference.
[14] Letter from Kenneth E. Kidd to the writer, Nov. 25, 1943.

smoothed and shaped with an elkhorn, this dough was put in the cavity and roughly plastered over the clay layer. When it began to dry out, two holes were punched in this inner layer opposite each other and near the top edges of the sides. Then a large stone, just the right size and shape to fill and form the inside wall of the pot, was placed in the cavity. Next a fire was built over the hole. It heated the stone very hot. After this firing had dried out the pot, it was removed from the hole by (1) digging away the top portion of the outer clay lining, (2) lifting the pot by the handle holes with the rock inside it, and (3) turning the pot over carefully and extracting the rock.

A second description of Blood pottery making was related by Mrs. Frank Racine, a middle-aged woman whose mother was a Blood Indian. Mrs. Racine said her grandmother had told her of Blood pottery making, as her mother before her had described it. Mrs. Racine believed that this was a description of the method of Blood pottery making in her greatgrandmother's time. That probably would have been during the first quarter of the nineteenth century.

The vessel was built up by hand. No mould was used. Then it was greased inside and out and placed over a fire to dry. This fire must not be too hot or the pot would crack. The vessels were quite thick and fragile. They were easily broken, so people had to be very careful with them. Two shapes of vessels were made. One was a cooking pot with a constriction near the top around which the handle was wrapped. The other was a flat dish.

Short Face, a Piegan in his middle seventies, told of a type of ceremonial pottery said to have been made by Chief Lodge Pole. He said that this aged chief of the Small Robes band died about 1894. Short Face declared that, though he had never seen any of Chief Lodge Pole's pottery, the old man once told him how he used to make it.

A sticky clay was obtained from around elk licks. The clay was mixed with a much smaller amount of sand rock pounded fine. The two were mixed with water into a dough. The dough was then put into a form made out of rawhide the exact shape desired for the outside of the dish. The clay was smoothed over the inside of the mould with the hands, to about an inch thickness. Then the whole thing was placed in the sun to dry. After it was dried, the dish was loosened from the rawhide, and the outside surface of the dish smoothed with a stone. Finally the outside was painted with red earth paint. These dishes were flat on the bottom. The sides were almost straight. Bottom and sides were about an inch thick. They were about 8″ across and about a foot high. These dishes were never placed on the fire, nor were they used to hold liquids. They were used only to hold dried meat or pemmican in serving ceremonial feasts. These dishes were not strong. They would fall apart in time. But they were much admired. Because they were so fragile much care was taken to preserve them from harm. For storage they were hung up in the lodge inside a rawhide slip cover gathered over the top with a drawstring. These dishes were always used by men.

Richard Sanderville (born in 1866), a mixed-blood Piegan, whose excellent

memory and knowledge of his people's past are widely recognized among the Indians on the Blackfeet Reservation in Montana, said of pottery:

I have been told that Piegan pottery was made of clay mixed with sand. It was shaped by hand. The pot was kept greased all over to prevent it from cracking, and hardened over a fire.

Probably the greatest weakness in the case for Blackfoot pottery lies in our inability to locate the corpus delicti—a genuine specimen of Blackfoot pottery. No writer on the Blackfoot has claimed to have seen a piece of Blackfoot pottery. However, two of our informants have said that they have each seen one vessel. Sanderville recalled:

I saw a cooking pot when I was a boy. It belonged to one of the wives of Many Horses, the Piegan chief, who was killed by the Gros Ventre in 1866. It was thick, flat-bottomed with straight sides, about 15 inches in diameter and about a foot high. It had two holes near the rim for a handle. When she moved camp she carried it in a laced rawhide container which was packed on the top of a packhorse's load.

The Blood Indian, Weasel Tail, stated

There used to be a shopkeeper in McLeod (Alberta), who kept a store on the south side of the street, at the east end of town. He bought up a lot of old Indian relics and kept them on view in his store. One of those relics was an old clay pot, found on the plains in the Blackfoot country. The last time I saw it was when I was about 28 years old (ca. 1885).

I saw that relic many times in company with other Indians. We used to look it over carefully and talk about it. I remember it clearly. It was about 12″ broad and about 8″ high. The sides sloped out a little from the bottom. It had two holes in the sides near the rim, but no handle in it when I saw it. The sides were a little thicker than the pencil you hold in your hand. (This was $\frac{1}{4}$″ thick.) It was the color of cement.

Our efforts to obtain further information about that vessel in the town of McLeod in the fall of 1943 met with no success.

Summary of the Direct Evidence on Blackfoot Pottery: Direct evidence in the case for Blackfoot pottery now rests upon Bradley's brief account of about seventy years ago, a statement in a myth, and traditions of Blackfoot pottery making obtained by three field investigators from some eleven Indians representing all three tribal divisions (Piegan, Blood and North Blackfoot), over the past four decades. To what extent is it possible to define the characteristics of Blackfoot pottery on the basis of this evidence? Since all accounts but one refer entirely or primarily to a cooking pot, we may consider it the most common article of Blackfoot pottery. Majority testimony indicates that it was made of a moist, sticky, clay paste tempered with sand; shaped with the hands (there is no mention of any tools); dried in the sun; rubbed all over inside and out with animal fat and hardened over a fire. The finished vessel was cylindrical in shape (i.e., flat bottomed with nearly perpendicular sides),

broader than deep, and of considerable thickness. It had two holes in the sides, opposite one another for insertion of a rawhide handle by which the pot was suspended, apparently from a tripod, over the fire. There is no mention of any surface decoration. That there may have been some variation in the form of the vessel and its method of manufacture is suggested by the minority testimony. Pulverized sand rock may have been used quite commonly for tempering material. There is some testimony from all three divisions that a mould of some description was used in shaping the vessel. Although there is little consistency in the description of the mould and its use, the testimony is too persistent to be ignored lightly. There is a single reference to a constricted rim cooking pot. The description of a mould-made, ceremonial serving vessel, cylindrical in shape, deeper than broad, by one informant, and the mention of a flat dish by another, are the only indications we have that the Blackfoot used pottery for purposes other than cooking. On the whole, these descriptions of Blackfoot pottery portray a thick, undecorated, friable ware, limited to a few elementary shapes.

The testimony throws some light on the time of discontinuance of pottery making by the Blackfoot also. It is apparent that the increased mobility of the Blackfoot pursuant to the acquisition of the horse did not drastically curtail the quantity output of pottery, for considerable pottery must have been manufactured for fully a century after horses were obtained. As Bradley has indicated, the furnishing of the Blackfoot with metal trade kettles must have been the primary cause of the abandonment of the pottery craft. Alexander Henry found kettles "scarce" among the Piegan in 1810;[15] while Culbertson estimated that there was about one kettle to every lodge in 1833.[16] The few datable descriptions of pottery making recently obtained pertain to that intervening period. It is very possible that the descriptions of the craft that have been obtained refer to its practice in a period of decadence not long before its virtual abandonment. Possibly Blackfoot pottery of an earlier period was somewhat less crude. It is probable that very little pottery was made after about the year 1825. When every household possessed a metal kettle that could be banged about over the plains for years without serious damage, the friable clay cooking pot had no further usefulness as an everyday utensil. That it may have survived for a number of decades as a form of ceremonial vessel, as indicated in Short Face's description of Chief Lodge Pole's pottery, is entirely plausible. The survival of objects once in common use, in the role of ceremonial equipment is not uncommon among the Blackfoot. Witness the ceremonial lance, and the wooden bowl which is still an integral part of the medicine pipe bundle.

Indirect Evidence from Neighboring Tribes: In 1892, when Grinnell expressed

[15] Henry and Thompson, 1897, p. 724. [16] Bradley, *op. cit.*, p. 256.

doubt that the Blackfoot had made pottery, little or no evidence had been published to indicate that pottery had been made by any of the nomadic tribes north and west of the horticultural peoples on the Missouri in the Dakotas. Since then the picture has altered materially. Some evidence of the former manufacture and use of pottery has been found among the majority of the nomadic tribes who were neighbors of the Blackfoot in the late eighteenth and early nineteenth centuries. In addition to the eyewitness account of Gros Ventre pottery by Mathew Cocking in 1772, traditions of pottery making have been reported for the Gros Ventre,[17] Sarsi,[18] Kutenai,[19] Northern Shoshoni,[20] Arapaho,[21] Assiniboine[22] and Cheyenne.[23] The Calling River People, a band of the Plains Cree, have also claimed that their people formerly made and used clay kettles.[24] Lowie found no evidence for the former use of earthenware vessels by the Crow.[25] However, it has been suggested recently that the pottery-bearing Hagen Site, near Glendive, Montana, may be prehistoric Crow.[26]

Because of the very fragmentary nature of the published descriptions of Gros Ventre, Arapaho, Assiniboine, Plains Cree, and Northern Shoshoni pottery it is impossible to compare it with the pottery of the Blackfoot. As might be expected, because of the long association between the Blackfoot and Sarsi, descriptions of their pottery bear a marked similarity. Kutenai pottery seems to have been even more crude than either of them. On the other hand the excavated pottery materials from the Sheyenne-Cheyenne site,[27] and the Hagen Site[28] are definitely representative of greater skill in the potter's craft than anything that has been attributed to the Blackfoot. The Cheyenne wares are said to resemble pottery from Minnesota sites. Hagen Site pottery is very similar to that of the Mandan.

Recently Will and Hecker have described a type of crude pottery from the Upper Missouri River Valley in North Dakota. The village sites give no definite evidence of agriculture, and were occupied at a relatively early date. "Probably the occupants of these villages preceded the Mandans but could have been contemporary with the first Mandan occupation in the area."[29] The potsherds are described as

> . . . very crude appearing, grit tempered, well fired, woodland type with rim top incised or with punched decorations predominating. From the small size of the rim

[17] See footnote 9, this paper. [18] Sapir, 1923, Vol. 25; Jenness, 1938, p. 14.
[19] Turney-High, 1941, pp. 77–78. [20] Lowie, 1909, p. 177.
[21] Kroeber, 1902, p. 25. [22] Lowie, 1910, p. 12. [23] Grinnell, 1923, p. 236–239.
[24] Skinner, 1914, pp. 79–80, 82; Mandelbaum, 1940, p. 214. [25] Lowie, 1922, p. 212.
[26] Mulloy, 1942, pp. 101–102; Will and Hecker, 1944, pp. 34–35.
[27] Strong, 1940, pp. 373–374.
[28] Mulloy, *op. cit.*, pp. 11–38. Will and Hecker, *op. cit.*, p. 34.
[29] Will and Hecker, *op. cit.*, p. 36.

tops recovered it is impossible to determine the rim or body shapes of the pots. The arc of the few rim sherds recovered shows a rim top diameter of 8 to 10 inches which would mean a medium sized pot of bowl shape, or a very large pot of constricted neck type. The entire surface of the pot is roughened by apparent grass rubbing or pressing, and in many cases the rough surface shows that grass stems, seed heads or something of the sort had been imbedded in the clay. Some sherds show vertical strokes, others horizontal. The surface finishing of these pots is entirely different from any other culture in the area and a few of the sherds have the appearance of having been formed inside a grass basket but that method of pottery manufacture cannot be ascribed to the culture. Unlike the pottery of the historic village Indians which is smooth inside the inner surface of these sherds is slightly irregular. These sherds while having the appearance of a poorly constructed ware are still of a very good quality and were used in cooking as the pot black on the sherds shows. The sherds are of gray color tones apparently open fired and a large percentage appear to have been highly sanded before firing.[30]

This evidence of a different, cruder and perhaps earlier pottery than Mandan in the Northern Great Plains is stimulating. The suggestion that a basketry mould was used in the manufacture of some of it adds significance to our descriptions of Blackfoot mould-made pottery. Is it possible that the use of a mould in the manufacture of pottery is a trait of some antiquity and breadth of distribution in the Northern Great Plains? Is the Blackfoot pottery that has been described a survival of a simple and crude pottery tradition, or is it a terminal, decadant form of an earlier, better made, decorated pottery? What are the historical relationships among the several nomadic tribes north and west of the Mandan in the matter of ceramics? These are all questions that must await further archaeological research over a much wider area of the Northern Great Plains than has yet been explored. To date a limited amount of archaeological research has been done in the whole area, and the great majority of that has been confined to the villages of the horticultural tribes. While we need no longer doubt the existence of Blackfoot pottery, we would wish for better knowledge of some of its implications. It is in the hope that the information may be valuable to future workers in the field that this review of the case to date has been prepared.

MUSEUM OF THE PLAINS INDIAN
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[30] *Idem*, pp. 36–37.

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BOOK REVIEWS

NORTH AMERICA

Old Oraibi, A Study of the Hopi Indians of Third Mesa. MISCHA TITIEV. (Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. XXII-No. 1, 277 pp., 4 plates, Cambridge, Mass. Published by the Museum, 1944.)

The present monograph is the result of four visits to the Hopi country since 1932. During his first stay, Dr. Titiev investigated "clans, lineages and related problems." During his second, which was the longest and covered a period from August 1933 to March 1944, he was able to participate to a considerable extent in the everyday life of the Oraibi community and to make an intensive study of Hopi religion. Part I of his publication is devoted to kinship and social organization, Part II to Hopi ceremonialism.

In the last fifty years a goodly body of data on both these topics has appeared in print, but while Dr. Titiev has incorporated much of this into his own material (with benefit, one might add, to the conceptual and historical aspects), his special interest in the dynamics of change as exemplified by the famous split in 1906 has disclosed much new and valuable information, especially since he probed behind the ready answer that the schism was due to a dispute over White American influence. The receptiveness of one faction to this influence indeed provided the *causus belli*, but, as he has pointed out, the primary reason for the split lay in the long-standing rivalry between two strong clans within the same phratry for ceremonial position and land (pp. 75, 79).

The long and disruptive conflict that preceded the break-up of the old village, and the way in which the new settlements were made gives considerable insight into the whole clan-phratry problem, a problem that has been a frequent subject of argument among Hopi specialists. Dr. Titiev's material and extensive discussion do much to clarify the structure of the Hopi household, lineage, clan and phratry, and the relationships between them. If, on the basis of only a few instances, he seems to reject too completely the possibility of separate clans merging to form a phratry, his own argument, following Lewis H. Morgan's suggestion "that phratries arise from the segmentation of clans" (p. 56), is nevertheless highly plausible. A final decision will have to wait upon further investigation, but any student interested in these and allied problems will find Dr. Titiev's discussion stimulating and rewarding.

Chapter V on the Amorphous State contains perhaps the most explicit statement obtained from any pueblo on the relation of the Village Chief to matters of land-ownership and land-distribution. The Emergence Myth, recorded by Voth and included here, gives the Village Chief (also the leader of the Bear clan) the necessary supernatural sanction (p. 61), and the sacred stone fetich (p. 60),which is in his care, pictorially expresses his control (and that of the other Soyal officers, the Pikyas, Parrot, Tobacco, Crier and War chiefs) over the Oraibi domain. While emphasizing the decentralized character of Hopi government, Dr. Titiev is careful to state that the political functions which concern the entire pueblo rest with "only two" chiefs, the Village Chief, "primarily by virtue of his stewardship of all the pueblo's lands" (p. 64), and the War Chief, who is "given the duty of maintaining discipline" (p. 65). All too often has the assump-

tion that ceremonial leadership and political power are in a one-to-one relationship obscured the basic structure of Pueblo society.

Dr. Titiev's realistic and detailed accounts of Hopi life also make it possible to assess the comparative strengths of permissiveness (as expressed in the parent-child relationship and in matters of sex) and the intimidating pressures of even so "decentralized and essentially non-legislative type" of government as the Hopi, pressures applied in early childhood (see scare kachinas, pp. 216 ff.), in later childhood (see kachina cult initiations, pp. 115 ff.), and in adulthood (see severe whippings, pp. 65 ff, and 115, and fear of witchcraft, p. 65 and note 46). Both aspects of Pueblo training have been commented on before, but Dr. Titiev's discussion again brings into focus the numerous and often contradictory factors involved in the Hopi socialization process. It challenges us anew to look beyond the parent-child relationships to the over-all aims of the society for an understanding of the basic personality structure. When viewed from this angle, "Hopi docility" no longer appears as an anomaly in a permissive milieu, but the logical result of the need for cooperation in a community depending upon agriculture in a semi-arid environment.

In the ceremonial descriptions in the second section, the author has again brought together relevant material from previous publications, noting particularly opinions or observations differing from his own. Each description is followed by a short passage which seeks to interpret the ceremony under discussion and integrate it into the total ceremonial scheme. Chapter VIII, The Basic Pattern and Underlying Concepts of Hopi Ceremonies, and Chapter XIV, The Scheme of Hopi Ceremonialism will be of great help to students of Pueblo religion.

Where so excellent an effort has been made to integrate new and old material, to argue specific points, and to offer stimulating interpretations, it is perhaps carping to criticize the general form in which the material has been presented. That form, Dr. Titiev tells us, has been dictated by his special interests. But it seems, at least to the reviewer, that the valuable contributions made in this monograph could be more easily comprehended, both by students of the Southwest and specialists in other areas, if, at the beginning, greater emphasis had been placed on the environmental and historical factors that made the complex patterns of Oraibi social and ceremonial organization necessary.

ESTHER S. GOLDFRANK

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MIDDLE AMERICA

The Aztec and Maya Papermakers. VICTOR WOLFGANG VON HAGEN. (Second edition. Introduction by Dard Hunter; x, 120 pp., 39 plates, 6 text figures. $6.00. J. J. Augustin, New York, 1944.)

Mr. Victor Wolfgang von Hagen first produced *The Aztec and Maya Papermakers* in 1943 as a limited edition, which was selected by the American Institute of Graphic Arts as one of the best books of the year. In 1944, with slight changes, the publishers brought out a trade edition.

The volume is handsome, the text interesting, and the appeal of the book will have a range extending beyond archaeology and anthropology into the realm of the bib-

liophiles. A great deal of research has gone into the book, which covers the ancient and modern use of paper in Middle America, and the sources of such paper. Mr. von Hagen lays the ghost that aboriginal American paper was made from the *agave*, and demonstrates completely and convincingly that it was made from the inner bark of various types of wild fig.

The book was written for an informed public rather than readers in segregated technical fields. As such, it perhaps should not receive the same rigidly critical scrutiny as if it were a basic technical work. Yet there is a certain looseness throughout the volume that suggests a lack of scientific self-criticism. Mistakes in the meaning of Aztec words, like giving, instead of *cempoalli*, the Aztec word for "twenty," the word *pilli*, meaning "son," seem inexcusable. To fly in the face of the pictographic evidence for *cuauhnahuac*, and translate it as "place of the eagle" instead of "by the wood," is careless, not to say negligent, of accepted usage in translation. The confusion as to the time of origin of papermaking, and the suggestion that it may have followed, rather than preceded, carving on stone by the Maya likewise seems uncritical. On page 46 he cites as a "Señor Gam" the scholar Antonio Leon y Gama, who, as a private citizen in the troubled years of the early nineteenth century, did so much to hold together the surviving remnants of Mexican manuscripts. This reference in connection with other statements seems to indicate a lack of detailed study as to the quantity and location of surviving Mexican picture manuscripts, the number of which seems fairly consistently to have been underestimated. These citations are not meant to be considered in a spirit of academic captiousness, but rather to give an impression of the trend of the volume.

The author has been through a large body of material, but he evidently wrote his book from his notes, which were hastily and carelessly assembled. Mr. von Hagen has amassed so much important and interesting information that it is a pity that it should suffer from careless checking of background material. Perhaps if the author had exercised more care his book would lack its present gusto. Perhaps, if the author were more the professional scholar, he would never have entertained seriously the idea of a general book on paper of interest both to bibliophiles and to Americanists.

On the other hand, this academic generation has seen an increased production of popular books wherein slovenly and distorted scholarship has been excused so as to promote popular appeal. In the long run, it seems difficult to see how a field of study can maintain its honor and interest when presented in terms of the jerry-built or brummagem. If Mr. von Hagen had put in six months more of work, there would have been no need to make this delightful book the subject of so generalized a reproof as is manifested in this review.

GEORGE C. VAILLANT

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Excavations at Tajumulco, Guatemala. BERTHA P. DUTTON AND HULDA R. HOBBS. With Appendices by T. D. Stewart and W. C. Root. (Monographs of the School of American Research, No. 9, xii, 124 pp., 6 maps, 101 figs., bibliography and index. Santa Fe, 1943.)

A three-month season of intensive work by the authors is here reported on and the resulting material is fully worked up. Excavation was for the School of American Research, carried on in 1938–1939. The site is in a region hitherto unsampled by a proper archaeological investigation, and well chosen as one of very great potential importance in working out inter-regional and temporal relationships in Middle America in general.

This alone would make the publication an important contribution, but its value is multiplied by the fact that plumbate pottery turned out to be characteristic, and that it was for the first time found *in situ* and in quantity—whole vessels, not merely sherds. This type was widely traded and is of prime importance in all attempts to reconstruct cultural time-sequence in Middle America.

There is an informative discussion of plumbate ware as such, in which the aid of Dr. Anna O. Shepard was sought and utilized. The term "quasi-plumbate" is introduced as a temporary term, apparently to allow for more refined future classifications and for possible limitations using different clay and slip. "Plumbate Group" covers plumbate proper and quasi-plumbate. The authors state that more than fifty per cent of whole vessels were of plumbate, but, in their terminology, quasi-plumbate is apparently included. I think a breakdown of the table listing the pottery found in nineteen out of twenty-one tombs would justify calling Tajumulco a *plumbate site*. I count a total of sixty-four pieces, six of which are utility ware. Of the remaining fifty-eight, twenty are plumbate, fifteen are quasi-plumbate—that is, there are thirty-five of the plumbate group against twenty-three examples from five other "ceremonial" types. The most numerous of these others, black and red-and-buff ware, only yielded six pieces each. The distribution and associations through the tombs is informative. Of eighteen tombs with non-utility types, only four lacked plumbate and only one lacked the plumbate group. In that one there were only two red ware vessels, that is, no association of types at all.

Because of this plumbate predominance, and of associations with it—including stone sculpture and, in the tombs themselves, a "fine orange" jar, and miscellaneous objects, some of copper—the site calls for full description and illustration, and this is admirably provided. For the stone sculptures, many of which are new discoveries, a plan to make casts miscarried. Drawings by Tejeda, by arrangement with Carnegie Institution of Washington, are to be published separately, also for practical reasons. The report places the site in local geography and gives notices of neighboring known sites. Local minerals and soil samples were collected and identifications and analyses are presented, while special appendices by the collaborators noted in the heading are devoted to the copper from a metallurgical point of view and to skeletal remains. The latter are scant, but include one reconstructed skull (illustrated). This shows a type of deformation which Stewart thinks is uncommon in this area but which he has detected in material from Cerro de las Mesas. Connections of one kind or another between the Pacific and Gulf coasts seem confirmed with a new sort of evidence which, as is noted, remains cultural, though the trait affects the human body. He voices a plea that archaeologists devote more care to poorly preserved skeletal material, implying that a good example has been set by the authors in this case.

The whole project has been well conceived and followed through. Under *Discussion* and under *Summary and Conclusions* (short sections) an attempt is made to indicate provisionally probable relationships, geographical and temporal. These can best be assayed by someone who has been working in the Guatemala highlands and to whom much recently acquired and as yet unpublished data are available. Since Dr. A. V. Kidder is one such and has done so in a review in *American Antiquity*, I will only note here that he agrees that the site belongs temporally in the Mexican period of the general Middle American sequence.

LINTON SATTERTHWAITE, JR.

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SOUTH AMERICA

Doll Play of Pilagá Indian Children. JULES AND ZUNIA HENRY. Foreword by David M. Levy, M.D. (Research Monographs No. 4, American Orthopsychiatric Association, xiii, 133 pp., 8 tables. New York, 1944.)

Psychologists and psychiatrists have utilized doll play for a number of years, both experimentally and therapeutically, ever since it was found that young children, who cannot adequately verbalize their feelings directly, will usually demonstrate them quite plainly when telling what a set of dolls is doing and thinking. A common procedure is to present the child with a doll to represent himself, and others of appropriate appearance to represent members of his family. These may or may not be named, but they are at least designated as "the mother," "the little girl," "the baby," etc. Then a conversation is held in which the examiner speaks for some of the dolls and asks the child what the self doll says and does. This is usually aimed at uncovering a difficulty the child is having in his interpersonal relationships. The child projects his own experience onto the doll group, and tells his own problems in his account of how the self doll behaves. Where the child's problem is known to the examiner, the conversation between the dolls can be used to make the child aware of it. If the child's problem needs to be clarified, the reactions which he spontaneously attributes to the dolls are used as cues by the examiner to orient the situation. Dr. David M. Levy utilized this technique to make young patients aware of the rivalry they felt for a younger sibling.

The Henrys' book is the most recent of the series of publications on their research among the Pilagá Indians of the Gran Chaco. It is a report of their experience with the technique described above when applied to the children of the village where they worked.

Their procedure consisted in Mrs. Henry's observing the children while they manipulated the dolls, and recording what they did and said. The set of dolls included a mother doll, father doll, baby doll, several "child" dolls, a turtle, a pair of scissors, and some plastiline. Ideally the children played one at a time, but frequently there were two or even more of whom Mrs. Henry had to keep track. After some children put plastiline breasts or genitals on the dolls in the first trials, this was made part of the experiment, and was suggested if the child failed to add these organs spontaneously. It was sometimes suggested that the baby doll be put to nurse. If genitals and breasts were left from a previous child's play and the next child's family had a different assortment of the sexes, it was suggested that the parts be altered to suit the new family line-up. Otherwise the examiner acted principally as sympathetic observer.

As the authors point out, this procedure differs in a number of respects from Dr. Levy's sibling rivalry experiments. For one thing, most members of the Pilagá child's immediate family were represented with the dolls, whereas Levy used only the mother, patient, and one sibling. For another, the play was relatively spontaneous and free, while Levy focussed the patient's attention on the rivalry situation. Under such circumstances, the results were more varied, and yielded data on other subjects besides sibling rivalry, particularly the patterns of hostility towards different members of the family, and patterns of sexual objects. This latter was shown by the way in which the child paired family members in sexual activity.

The monograph includes a sketch of some of the main cultural factors having a bear-

ing on the experiment, observations of sibling rivalry outside of the doll play, a careful description of the experimental procedure, an analysis of the data, a chapter on behavior problems, another on sexuality, a couple of pages of conclusions, and finally the protocols of the experiments with the behavior of each child on different trials grouped together. There is enough material in the volume for an understanding of the experiment and some of its implications, but in addition the Henrys have generously provided data so close to the original form that it can readily be compared with field notes of other workers. Throughout there are critical comments on the procedure and results, comparisons with Levy's work, and questions posed by the experiment.

The authors undertook their field research with the object of testing two propositions: (1) That, given similar intra-familial relationships in distinct cultures, the effects of these relationships on the children will be similar. (2) That, wherever sibling rivalry exists the general symptom pattern will be approximately the same.

Their principal conclusions are:

The study of Pilagá intra-familial relationships shows that under similar conditions of familial tension children in distinct cultures will develop the same kind of symptom patterns.

As in almost any piece of research, other useful data were obtained incidentally to testing the fundamental hypothesis. Outstanding in this connection is the material related to the experimental procedure itself.

Knowledge of the language and culture of a tribe is necessary before valid interpretation of the experimental data from a primitive tribe is possible.

The monograph is an unassuming, straightforward presentation, with only the soundest deductions made from the material, and there is ample opportunity for the reader to judge them for himself. The Henrys state that their purpose in presenting this material is "to provide psychologists, psychiatrists, and anthropologists with a reference for a comparative study of certain types of problems, and to present a demonstration of a technique in anthropological field work." They have accomplished this aim admirably, and in addition have contributed a model for reports of this sort of work. The book is very stimulating to anyone interested in methods of studying personality reactions in cultures other than his own. The manner in which the protocols have been organized and tabulated should be suggestive to other field workers.

This is not intended in any way to be a definitive description of Pilagá child life. Yet it is tantalizing to learn as much of them as we do here and still have only a partial picture. The authors promise to tell us more, and I hope that their next publication will be an integrated account of these Indians which will bring together everything the Henrys know about them.

DOROTHEA C. LEIGHTON, M.D.

OFFICE OF INDIAN AFFAIRS

AFRICA

The Succession of Bemba Chiefs. A Guide for District Officers. W. V. BRELSFORD. (iv, 43 pp. 3/6. The Government Printer, Lusaka, Northern Rhodesia. 1944.)

The attention currently being given to the political systems of native Africa bids fair to make of them the best known of the primitive world, and may even achieve the distinction of bringing them under the scrutiny of political scientists, as well as of an-

thropologists who happen to become interested in problems of government. Mr. Brelsford, writing from the point of view of a colonial administrator, and addressing himself primarily to the problems a colonial official must face, has nonetheless achieved a piece of scholarly work in this field to which any professional student of nonliterate cultures might be glad to lay claim. His eye is always on his principal problem—what are the pertinent facts concerning the Bemba chieftainship that must be taken into account by the Colonial Government in directing the complicated succession to the Paramountcy and lesser chieftainships? But this is done with such a wealth of documentation, and with such clarity in exposition, that the result must also rank as a study of first-rate scientific importance.

For background to this work, one should refer to various books and papers of Audrey I. Richards, especially her chapter "The Political System of the Bemba Tribe" in the volume *African Political Systems*, edited by M. Fortes and E. E. Evans-Pritchard. Yet it is also salutary to allow the impact of the complexities of the Bemba succession to be felt by reading this analysis without the benefit of background materials. A striking picture is drawn of institutional stability, about which center the overtones of intrigue and other types of maneuvering for position, that result in shifts of authority. It is not difficult to comprehend and, in a sense, sympathize with the feeling of the author concerning the importance, as a purely practical matter, of this complex system being fully grasped by those who must deal with it. For if it is not understood, it is apparent that they will become hopelessly enmeshed in its confusing details.

As among many African peoples, royal genealogies are well known and carefully remembered. The Bemba inherit position within the royal clan by a series of steps, the Paramountcy and the most important Chieftainships going in the female line, succession through the father being only to less important and distinctly non-royal posts. Generation is crucial as well as descent, so that according to rule, the first Paramount in a given generation, who bears the title of Chitimukulu, must be and usually is the eldest son of the eldest *namfumu* (female sibling) of the preceding generation. A Paramount is succeeded by his next elder brother or other collateral, who until that time customarily holds the post of Mwamba, though he may graduate from the Nkula chieftainship to the Paramountcy. The point of the system is that the naming of a new Paramount means that there is a shift all along the line in the lesser posts, and a consequent movement of chiefs from one seat to another.

The reading of this closely knit discussion is facilitated by the excellent charts appended to the text, which give the genealogy of the royal line for the past six generations —that is, since the first part of the nineteenth century—and the succession in the male line. The use of letters and numbers to facilitate reference is a technique that could be adopted more generally by students of social structures to advantage. The chart, as it stands, is abbreviated—understandably, since a "more thorough" one compiled in 1910–12 was some eleven feet long! The present one measures only four feet, but it includes all the principal figures of the recent historic past.

The death of the Paramount Chitimukulu VIII while this booklet was in press gives immediate point to the discussion. "At least a year will pass after the death before the successor begins to inherit," we are informed, "and, as Mwamba too has been dangerously ill for some time, it is likely that there will be great changes of personnel throughout the main lines of Bemba chieftainships within the next year or so." Since the in-

heritance of a "big chieftainship" takes at times as much as two years, the succession should become resolved at about the time this notice appears. It is to be hoped that Mr. Brelsford will find time to record the moves that are now taking place, and put them on record in a later publication with the same meticulous care for detail that has marked the work under discussion here.

MELVILLE J. HERSKOVITS

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ASIA

The Ageless Indies. RAYMOND KENNEDY. (xvii, 208 pp., 3 maps. $2.00. The John Day Company, New York. 1942.)

The Ageless Indies is far more than the conventional compilation of facts and figures which makes up the ordinary handbook. The author has been engaged for several years on a comprehensive account of the native peoples of the Far East. He here presents a summary embodying the salient facts about the Indies, including an acute analysis of Dutch rule and some important remarks on its future role. For this area, with over seventy millions of population and rich resources, will be one of the focal points of attention in the Far East after the war.

Professor Kennedy's primary concern is with the peoples of the Indies—the natives who have withstood successive intrusions into their islands "without losing their own cultural and social identity." His chapter on "The Indonesians" is the best summary of Indonesian culture so far available in English. The whole region is covered—not merely Java and Sumatra—and the author points out the great variety and range of custom in what he characterizes as the "greatest existing 'native' region in the world." There are some 130 native groups in the Indies, ranging in size from a few hundred to several millions. The long and complex racial and cultural history of the area has brought about a situation which should be of great interest to students of social and cultural change.

The virtues and defects of Dutch colonial policy are well brought out. Kennedy states: "There is no doubt that the Dutch civil officers in the East Indies were the best colonial administrators in the world," the reason lying in the methods of selection and training. Yet basic policy with regard to such important matters as education and self government was not up to this high standard. Of the electoral procedures he notes that

> The whole process was amazingly complex and cumbersome. One is tempted to pass the judgment that the Dutch moved so slowly and deviously in the whole matter of native representation that they had tangled up the natives—and themselves—in a needless mess of red tape" (p. 117).

This is giving the Dutch the benefit of a considerable doubt, since analysis of the whole system suggests that the procedures might have been developed to prevent any real native democratic representation.

In the last fifteen pages Professor Kennedy summarizes his views concerning the future of this important area. He proposes that "an end be made of the traditional colonial imperialism and that the former profit-making reservations, with their passive populations, be transformed into independent, self-contained counties, with free, self-governing, well-educated citizens" (pp. 189–190). For the Indies he would give the Dutch the task of directing this program. Beyond that he envisages a Far Eastern federation on an economic basis.

The Dutch will soon have another chance. Their responsibility in Indonesia is greater now than it has ever been in the past, for the Indies were one of the great prizes of war in the Far East and may easily become so again. We need to keep in mind the facts and evaluations here presented in assessing the official Dutch program as it is developed.

FRED EGGAN

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MISCELLANEOUS

The Origin and Function of Culture. GÉZA RÓHEIM (Nervous and Mental Disease Monographs. 107 pp. $2.50. New York. 1943.)

Róheim's theory of why and how civilization originated further extends Freud's point of view, and is of interest to all those who are aware of the unconscious motivations and levels of human behavior and have knowledge of the mechanisms revealed by psychoanalysis. The theory has the strength derived from pushing one point of view with great keenness; and it has the weakness of dealing with such large and complicated problems as the origin and function of culture on only one level, the unconscious; and further, restricting that level to one point of view. The reviewer's assumption is that the origin and function of culture are multiple and have a number of determinants, conscious and unconscious. Be that as it may, the book is a stimulating presentation of one theme which we will attempt to summarize, and then present a critique of the theory.

The prolonged infancy of man is the biological cause of culture which Róheim discusses, although he does not claim it to be the only one. The author uses an analogy between the psychology of growing up and the cultural process. He says, "One of the mechanisms of growing up is displacement or series formation" (p. 28), but "we only grow up to remain children" (p. 31). An example is given from Normanby Island to indicate that the growing-up there is a restoration of the infantile situation with its omnipotence. In this process the love received from parents is transferred into praise from adults. "Fame and praise are socialized equivalents of love" (p. 36). Continuing, "The life of an infant consists in a series of frustrations which commence with birth. He has to bear the loss of intrauterine happiness; he learns what it means to desire something without receiving it. He can tolerate suspense. At this juncture the road is bifurcated. One road is determined by the substitute objects which the infant finds in his own body. The other road is extrovert, it finds substitutes in environment; the clutching becomes a seeking. Instead of sucking his thumb when he can not hang on to his mother's nipple the future esa-esa of Duau or Dobu seeks new alliances, seeks to spend his whole life suing others for their gifts and in being sued by others" (pp. 36–37). Thus, the ideals of a culture "are based on the infancy situation on that peculiar aspect of human nature *which is rooted in our delayed infancy*" (p. 38). "Human groups are actuated by their group ideals and these are always based on the infantile situation. . . . We find substitutes for the love objects of infancy but under the veneer of giving love we always retain the desire to receive love, and the loves and triumphs of adult life are really 'Paradise Regained,' the refinding of the infancy situation on another level" (p. 39).

Róheim says the prolonged infancy of man is retardation, which he thinks is also responsible for neurosis. "Instead of cathecting the present the neurotic is fixed to the

past" (p. 21). All primitive societies were led by medicine men who are neurotics, and the author regards the medicine man as the first profession, apart from the "professions" of hunter, fisherman, and gatherer of berries and fruits. The profession of medicine man is evolved on the basis of infantile body destruction phantasies, by means of a series of defense mechanisms. The medicine men play their game based on their infantile anxieties, and relieve the anxieties of the others who have the same phantasies but to a lesser degree.

In a discussion of economic life, there is the same theme of the role of phantasy in shaping the economic situation. The hypothesis for the origin of the trader is the phantasy of the exchange of body contents—a mutual mother-child situation. Gardening is supposed to originate "from the anger (body-destruction phantasies) of a child who has suffered a deprivation. With the restitution phase of the sadistic trend comes planting, the caressing of the yam, the neat and tidy aspect of the garden and ultimately the actual providing for the future, for the next generation" (p. 58). This explanation, of course, applies to the latent meaning of the game. "Playing at gardening precedes actual gardening in the life of the individual and myth regards child as the inventor of the art" (p. 59).

The same type of hypothesis underlies the first use of the plough and the ox, the dog as man's first companion, and the domestication of cattle. The latter, according to Róheim, was brought about by cattle totemism or the displacement of the Oedipus complex to cattle. "The bovine herd is a reduplication of human society based on the oedipus complex, and the reverence paid to cattle is due to Father Bull and Mother Cow" (p. 66).

The last heading of Róheim's thesis is "sublimation and culture," and he begins with the difference between neurosis and sublimation. "A neurosis isolates; a sublimation unites. In a sublimation something new is created—a house, or a community, or a tool—and it is created in a group or for the use of a group. We must therefore assume that the same mechanisms which under certain circumstances are active in the formation of a neurosis are also behind our various sublimations, plus something else that is super-added. This 'something else' must be responsible for the trend towards the group which is so characteristic of sublimation" (p. 74). The author notes the violent oscillation between the narcissistic and the object-seeking attitudes, and "suspects that in the mechanism of normalcy, in sublimation this oscillation must dissappear and something more stable take its place" (p. 75). "Our theory therefore is *that the cultural object or a sublimation is half way between the narcissistic and the object-erotic position, a stabilization point for the oscillation of libido.* The great danger against which mankind has evolved culture is that of object loss, of being left alone in the dark" (p. 77). The child is identified with the cultural object. "*Defence systems against anxiety are the stuff that culture is made of* (p. 81), and ultimately, "*civilization is a series of institutions evolved for the sake of security*" (p. 84). "Our delayed infancy and permanently infantile nature determines a succession of situations modelled on the early infancy pattern" (p. 76).

In his conclusion, Róheim seems to indicate that primitive societies fulfill their function better than our modern more complex ones. In Central Australia, civilization is based on puberty rites and "the tjurunga is the symbol both of personal immortality (reincarnation) and of social cooperation" (p. 97). The natives of central Australia thus receive "the real security that lies in social cooperation and the security against object-

loss, personal immortality" (p. 98). Civilization consists in the development of new substitutes, without giving up the desire for the original. "But, and this seems to be the real tragedy of mankind, as the substitutes increase in quantity and area, they decrease in quality, they become more and more remote from a 'good object,' from a yielding nipple. As Lowie has shown, primitive man solves his simple task of living in a small society relatively well while civilized man living in great units has a far more difficult task and succeeds but poorly. It is doubtful whether any one can have the same feelings for the community which he entertained as an infant for his mother. The difficulty of the task lies in the inadequacy of the object. Man is constitutionally attempting the impossible" (p. 99).

The above summary cannot do justice to all the details of Róheim's thesis. But we have tried to present his main argument.

There are certain general points to which we think most everyone would agree, such as that "civilization is a series of institutions evolved for the sake of security," and certainly the prolonged infancy of man is regarded universally as one of the necessary conditions of culture. Many of those who understand the unconscious mechanisms revealed by psychoanalysis will also agree, either wholly or in part, with many other points made by Róheim, such as, the mechanism of displacement which occurs in growing up, fame as a socialized equivalent of love, the difference between neurosis and sublimation, the analysis of the function and origin of medicine men, neurotics who relieve the anxieties which the members of the tribe have in lesser degree than the medicine men, and to the over-all point of much of the unconscious symbolism in the functioning of culture.

But there are also a number of rather fundamental points to which the reviewer takes exception. We do not regard the prolonged infancy of man as retardation, but rather as a time for development and growth, both biologically and psychologically. We do not regard symbolism as the only origin, but only as one part of any hypothesis of origin. For instance, we think that the invention of tools is more complicated than Róheim's theory that they "are things to hold in one's hands (grasping mother), and projections of one's own organs" (p. 95), although that may be the unconscious symbolism. We would certainly have to add certain facts from prehistoric discoveries which show that the erect posture of early prehistoric man freed the hands from locomotive function and at the same time supplied the brain with the opportunity of expanding. The brain, particularly the cerebrum, took a progressive course, increased in volume and enlarged the association centers. Man's capacity to think, and to think symbolically, plus the ability to use his hands must be part of the story of the invention of tools, and of the development of culture which provides a different type of adaptation to environment than that used by any other organism. Certainly the prolonged infancy influences the form the symbolism takes.

But we do not regard it as the only influential factor in form. We can take an example from contemporary society. We think of Hitler as a kind of medicine man, and his function is rather much the same as those in primitive societies. He is obviously a neurotic and has, in exaggerated form, anxieties (infantile and otherwise) which the masses of German people (and many other people, too) suffer from. The defense mechanisms which he built up for himself and dramatized so well succeeded also in meeting the psychological needs of the masses of Germans who suffered from the same anxieties, in

lesser degree. This is, assuredly, one cause for Hitler's rise to power and the form of Nazism. But there are, obviously, historical, geographical, economic and other social factors which are also "causes" for the development of Nazism and of Hitler's success in seizing power.

The psychoanalytical point of view is extremely useful in revealing something about origin and function. It seems to us that it can not be used exclusively any more than any other one point of view can be used exclusively, whether it be historical, or functional, or geographical.

Another basic point of difference with Róheim is with his theory that human nature is *permanently* infantile, and this is connected with his viewing the prolonged infancy of man as retardation. If we accept Róheim's theory that the real tragedy of man is that, as his substitutes increase in number and size, they give less satisfaction, and that therefore man is constitutionally attempting the impossible by enlarging his sphere of objects to replace the original ones, then it will be impossible to have any form of international or even national organization which can engage the loyalty of human beings. Of course, the feelings of the individual for the community are not exactly the same as are his feelings for his mother, but that does not mean that they cannot have great strength and depth. The course of history seems to go against Róheim's theory. For we have seen the gradual enlargement of the area of man's loyalty—from the family and clan to the tribe and nation, and now the beginnings of an internationalism.

We do not see how any human situation, infantile or otherwise, can be *permanent*. Growth and change appear to be just as much a characteristic of man and of culture as is the power of symbolism and the prolonged infancy of man. Of course, man will always be seeking security, always need to be loved, always want to be omnipotent, and these needs have their prototypes in infancy. But we cannot completely agree with Róheim's theory that "we only grow up to remain children." There is a difference. We use an analogy from a comparison of primitive and modern society. Primitive man gains some security through his magic, i.e., through his fancied control of his environment. Modern man likewise gains some security through a control of his environment, but this control is based on science rather than on magic. Whether the control is real or fancied makes an important difference in its nature, and the two are certainly not the same, even though both, to some degree, have the same psychological function. The baby's fancied omnipotence arises out of his very helplessness and weakness; any degree of real power which the adult gains arises out of strength—again quite different. And adults are able to love as well as to crave love.

We agree with Róheim that human groups are actuated by their group ideals. But we have some reservations that the ideals of a culture are completely "based on the infancy situation, in that peculiar aspect of human nature which is rooted in our delayed infancy" (p. 39). We do not see how Róheim's theory can account for changes in group ideals. The concept of the freedom of the individual, of his relationship to authority, has gone through many changes from the primitive society of central Australia to American democracy. If we were *permanently* infantile, it is hard to see how we could gain any degree of independence from parental authority—a degree of independence which is represented by the democratic form of state. We, of course, do not deny the very great influence of our infantile bonds, nor the depth of their influence as we grow. But neither would we deny the desire to escape from them, and the slow but percep-

tible change in the individual's growing up and in the development of culture. We are unable to foresee that man will ever become completely secure, or that he will ever be truly grown up, but that does not mean that he must be *permanently* infantile, or that he does not grow up at all.

We do not think of any process in man or in culture as being *permanent*. We think of culture in Malinowski's sense as an instrument with the capacity of developing new ends and with creative possibilities. We know that man is still going through biological changes. We know also that there are changes in cultural forms and that there are laws underlying cultural change. Our hypothesis is of a trend in the psychological development of man away from his infantile insecurities rather than an exact return to them.[1] And we do not agree with either Lowie or Róheim that primitive man succeeds better than modern one in his adjustment to his environment. We refrain from going into a long discussion of what is meant by "better."

Much of the current anthropological work which makes use of psychoanalysis stresses the impact of culture on the individual. Róheim goes in the opposite direction, showing the impact of the basic infantile situation on culture, which is, of course, the Freudian position. We regard both as important, and think of the relationship between the individual and culture as a two-way process, rather than exclusively one way or another. Róheim concentrates only on one way, but he does it with keenness, brilliance, and originality. We get from him a deeper insight and increased understanding of the unconscious motivations underlying man's behavior, and of the enormous role the defense systems against infantile anxieties play in the functioning of culture.

We have stressed, as is the custom of reviewers, our differences from the author, and the limitations of a completely one-sided Freudian point of view. But we can learn much from Róheim's book. We said before that no one point of view used exclusively can answer such complicated questions as function and origin of culture. But sometimes we think we learn most from men of some originality who go to extremes in their point of view, even though we do not go all the way with them. They are usually the men who provoke and stimulate us, whose work has an illuminating quality. Although we can not accept Róheim's thesis in its entirety, we think it is a contribution to the endlessly fascinating, but perhaps unsolvable, puzzle of the origin of culture, and, what is more important to us, for the insights into basic unconscious motivations underlying much of the functioning of culture.

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[1] In *Escape from Freedom*, Fromm offers a hypothesis of man's development towards freedom, even though at the same time he desires to escape from this trend.

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BRIEF COMMUNICATIONS

INDIAN MYTHOLOGY AND EXTINCT FOSSIL VERTEBRATES

In a brief communication in this journal, Dr. Ashley Montagu gives an account by Albert Koch of an Indian tradition suggesting knowledge of the mastodon.[1] This account was published in 1841. Dr. Ashley Montagu suggests that it is "perhaps the earliest record of an Indian tradition concerning an extinct mammal." In addition he quotes an unnamed paleontologist as inclining to the view that the horse never became extinct in America. Dr. Ashley Montagu further indicates that the validity of these Indian historical traditions has been generally accepted.

Since these views, along with the whole subject of Indian traditions about extinct beasts, are highly controversial, I should like to venture a few remarks on the subject, and to contribute some additional material to the history of science.

Albert Koch is not the earliest writer to record Indian traditions regarding extinct mammals. Thirty-five years before, in 1806, Thomas Ashe had published an account of a tradition purporting to come from a "Shawanece" Indian.[2] A quarter of a century prior to Ashe's publication, Thomas Jefferson had given an account of the same legend in his *Notes On Virginia* published in 1782. This tradition was interpreted by Ashe, drawing from George Turner and Thomas Jefferson, before him, as referring to "Megalonyx." Like Turner and Jefferson, Ashe interpreted "Megalonyx" as a gigantic carnivore. His pamphlet suggests that his "Megalonyx" was a composite which he had created by combining sloth claws with mastodon teeth, and imaginatively reconstructing a giant lion twenty-five feet high and sixty feet long! Paleontology, one must remember, was in a very naive state one hundred and thirty-eight years ago. Ashe notes that this perfectly imaginary beast is alleged by the Indians still to exist "beyond the lakes."[3]

This intriguing little pamphlet is, if one reads between the lines, exceedingly revelatory of the interest which, even at the close of the eighteenth century, was beginning to animate those who had glimpsed fossil bones in the New World. Here, because of a small population, vast unpeopled wildernesses, and no intensive agriculture, fossil remains were much in evidence. Since this was long before Darwin, and before our knowledge of the glacial events of the Pleistocene, *every attempt was being made to correlate these remains with at least moderately recent events or with Biblical behemoths.* We may presume, says Ashe, "that the wise Creator of everything would not suffer so great a link in the chain as the Megalonyx to be entirely broken off. He continues every created species nor can they cease while the earth remaineth."[4]

We cannot comprehend the myth-making proclivities of this era scientifically unless we keep the intellectual climate of the age in mind. "Very true," the reader is apt to agree, "but what has this to do with Indian myths of this period?"

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[1] "An Indian Tradition Relating to the Mastodon," *American Anthropologist*, vol. 46, No. 4, pp. 568–571, 1944.

[2] Ashe, Thomas, *Memoirs of Mammoth and Other Extraordinary and Stupendous Bones*, printed by G. F. Harris, Liverpool, England, 1806.

[3] *Ibid.*, p. 49. Ashe's paper is interesting, not for its originality, but because of its transparent revelation of both the scientific weaknesses and popular interests of the time. Not even Jefferson conceived of his "Megalonyx" unaided. Such ideas were in the air.

[4] *Ibid.*, p. 35.

Two things, it could be pointed out, are involved here: First, it is plain from the discussions in the pamphlet that white men, actuated by curiosity and religious bepuzzlement as well as ignorance of interior North America, were avidly questioning Indians about these giant bones.[5] This means, ethnological techniques not having come into practice, that explorers were probably getting pretty much the answers they sought out of the vast animal mythology of the natives. No one who remembers the story of the desperate Spanish search for mythical golden Quivira should be surprised at this.

Second, the Indians themselves were certainly not immune to curiosity, particularly when fossil marvels were pointed out to them.[6] To men with no knowledge of geology, no knowledge of the great time stream of which we of to-day are so intensely aware, would not every bone, even if strange, be that of a mighty beast still living in grandfather's day or perhaps even now existing somewhere "across the lakes" or over the farthest hill?

Almost as if to document the subject further, Ashe comments that the Siberians assert that the mammoth lives underground. Again we see the primitive myth-makers at work in an area where frozen carcasses lend an even more vivid reality than bones. Must we say on this basis that the Siberian folk knew the living animal? Obviously not.

Finally we must remember that though these early accounts have been reported to us by men of our own race, their time conceptions were, geologically, little clearer than those of the natives whom they interviewed. Interesting as these early data are, they will never establish, unaided, the time of the extinction of the Pleistocene mammals.

Some time ago I endeavored to point out what would be necessary and desirable to clinch and demonstrate recent survival.[7] Dr. Ashley Montagu's reference to the possibility of the survival of the horse reminded me that I had had more than one campfire conversation with the late Dr. Edgar Howard on that very subject. He was somewhat inclined, at one time, toward this view. I argued that if the horse survived, it was strange that its bones had not been found in archaeological sites, from known cultural horizons. "Well," Dr. Howard countered, "perhaps it wasn't eaten."

Now the horse was eaten in paleolithic Europe. It was eaten, apparently, on the Folsom horizon in the New World. I submit that for it to have been alive and completely ignored by the American aborigines of later times flies in the face of all our knowledge of the world's hunting peoples.

The only way that a scientific demonstration of recent survival of the Pleistocene

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[5] G. G. Simpson has called attention to a letter by James Wright (1762) which refers to a Shawnee tradition involving giant animals and giant men. After analyzing the data, Simpson's comment runs as follows: "They [the Indians] knew there was no reliable testimony or memory of the animals in life. This contrasts significantly with legends later given by Jefferson, also as coming from the Shawnees, and believed by him to prove that the species of animals represented by the bones was still alive." (George Gaylord Simpson, "The Discovery of Fossil Vertebrates in North America," *Journal of Paleontology*, vol. 17, p. 36, 1943.) This statement by one of our foremost paleontologists does not, I think, validate Ashley Montagu's comment that the legends "have been generally accepted."

[6] Simpson, *op. cit.*, p. 27; Kindle, E. M., "American Indian Discoveries of Vertebrate Fossils," *Journal of Paleontology*, vol. 9, pp. 449–452, 1935.

[7] Eiseley, Loren C., "Archaeological Observations on the Problem of Post-Glacial Extinction," *American Antiquity*, vol. 8, pp. 209–217, 1942.

fauna can possibly be accomplished is by getting clear and authentic evidence of that fauna in association with human horizons which are dateable. It may be done. But so far the extensive excavations carried on by the University of Nebraska in a key area of the Plains and over a great vertical range of Recent time have yielded no horse remains below the protohistoric horizon. These facts, as I have pointed out elsewhere, cannot be airily dismissed while myth is brought forth as evidence. Somewhere the Pleistocene and Recent fauna interlock. That horizon must be found. Only so can the problem be settled in a manner satisfactory to those of us who demand artifacts and bones as evidence.

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THE CEREMONIAL DESTRUCTION OF SICKNESS BY THE WISCONSIN CHIPPEWA

A rather unique employment of sympathetic magic by the Chippewa is the practice of avoiding an impending plague by the destruction of a straw man made for the purpose. By the act of obliterating the straw man it is believed that the coming sickness is obliterated, on the principle that "like produces like, or in other words, that an effect resembles its cause."[1]

The ceremonial pattern is simple. A person is warned by his guardian spirit through the medium of a dream that sickness is about to descend upon the community. The person is always a man, and one recognized as having important powers secured through his fasting dream. He notifies the community by sending a runner with tobacco, the common method of invitation. The runner presents some of the tobacco (a bit of cut plug, or Standard) to each family, and tells them when and where to come, and to bring equipment for what they are going to do (*wimǝsinǝ̄šk!bijigǝn*—we are going to make an image). At the appointed time the people assemble, bringing food and tobacco, the men with guns, the women and children with knives, clubs, and axes. The Dreamer then tells the people his dream and why they are doing this. The food is laid out on the floor, tobacco is passed around and smoked, and the Dreamer gets up and dedicates the food and tobacco to the *manidog* (spirits) and asks their help in doing this thing. The people then eat, it being believed that the food and tobacco are really offerings which go to the *manidog*. After the feast the men take their guns, and the women and children their clubs, knives, and axes, go outside and cautiously approach the straw man which has been set a short distance away from the house by the runner. The figure is made of straw or hay (to be inflammable), varying in height from about two to four feet, and dressed in a miniature man's costume. It is either made by the runner the night before, or by the women just before the ceremony begins.

As the people approach the straw man the Dreamer gives the signal to shoot it with their shotguns, he joining them. The women and children then rush up to club it, cut it, and chop it to bits. The remains are gathered up by the people or runner, placed in a pile, and burned. They may return to the house where the Dreamer thanks them for their assistance.

[1] Frazer, J. G. The Golden Bough, pp. 37.

The principle of sympathetic magic is not foreign to the Chippewa. It is, however, usually a secret, individual undertaking in which a shaman is employed for killing, curing, or compelling behavior, especially in the realm of love. For such purposes a medicine doll is commonly used. It is a small human image carved of wood, often with squares cut into the front or back into which medicine is placed. The shaman will lay the doll out with the appropriate medicine, sing the song he was taught by his spirit, and speak to his guardian spirit asking his assistance. If he desires to kill a person he may bury the doll in the ground, shoot it with an arrow, or burn it. By "killing" the doll the person is killed. In curing, the doll is laid out, the song is sung, and the doll given instruction as to what to do. In the event that an unsuccessful lover enlists the services of a shaman, two dolls may be bound together symbolizing the actual union, with one of the dolls containing a bit of clothing or a hair from the person desired by the intending lover.

Thus while ordinarily sympathetic magic is a secret, individual undertaking, in the case of the destruction of the straw man it is a public, community venture, for a benevolent purpose. I have selected the following cases which were witnessed by two of my informants:

When I was living down at Sand Lake (the St. Croix Band of Chippewa), old J. K. had a dream that sickness was coming. This was about 1915. He told the people that there would be a feast the next day. About 15 or 16 people came to his house the next day. The night before he had made a dummy out of straw. It was about three feet tall, and in the image of a man. He had dressed it in a shirt, coat, pants, hat, and moccasins. When the people got there J. K. passed tobacco. He told them to light their pipes and smoke, and that while they were smoking he would tell his story.

He told them he had dreamed that a serious sickness was coming. His (guardian) spirit had appeared to him in the form of a man, and had told him that the sickness could be avoided by making a dummy and shooting it. He then spoke to his spirit offering this food and tobacco to enlist its further aid in keeping the sickness away. He also offered the food and tobacco to the four winds and other *manidog*. He invited all the people to eat, and after the feast he went out and was gone about five minutes. He came back very excited and reported that he had seen a spy (*nidamânis*) nearby. (The dummy had been placed just over a hill about six rods away that morning before sunrise.) All the people acted very excited at this news, although they really knew what it was all about, and what was going to happen. J. K. told three or four of them to get their guns and go out and shoot this spy. The men took their shotguns, and a few old ladies took their axes and they crept out and sneaked quietly along. They asked J. K. where he had seen the spy and he pointed to over the hill where the dummy was. They crept up to the top of the hill where J. K. told them to wait while he sneaked up ahead as an advance scout to determine the spy's position. Then he came back and whispered to the others that the spy was still there . . . just over the hill. He cautioned them to be quiet, and they advanced all abreast, slowly and quietly. When we (I was one of them with a gun) got close enough to see the dummy's hat, he told us to take aim and shoot, and we all shot the dummy at the same time. Then he told the old ladies to go down and finish him up, so they rushed down and chopped up the remains with their axes. They then gathered up the remains and burned them.

We all went back to the house and J. K. made another speech saying that he was glad all the neighbors helped him to carry out the instructions of his dream, that he hoped that his dream was true and that it would keep that sickness away.

I never believed much in that sort of thing, but J. K. asked me to help, so I did. They believe that by destroying that dummy they destroyed the sickness that was coming. J.B.

The present war has had some interesting effects on Chippewa life. The following case concerns J. S., a Mide Priest and religious leader. He had heard (he can neither read nor write) that there was a lot of sickness (malaria) among our troops in the Southwest Pacific. He dreamed that he should help the soldiers down there by destroying a straw man to alleviate the sickness. The following is an account by one of the younger participants in the ceremony:

Last November (1943) J. B., a runner for J. S., brought tobacco to our house and told us to come to J. S.'s house the next day, and told us what they were going to do. We went over the next day and my mother helped the women make the straw man. It was only about a foot tall made of straw tied together with string, and clothed with a little shirt, pants, and a headband. When it was finished they brought it in, and put it by the food which was laid out on the floor. Tobacco was passed and J. S. got up and spoke. He said they were doing this because of the soldiers having all that sickness in South Pacific (where his son is stationed). He then offered the food and tobacco to the *manidog*, and then we ate. After the feast J. S. talked to the straw man telling it not to stay around where Indians or soldiers were, and told his runner to set it up out in the field. The runner took it out and propped it up against a rock about 200 feet from the house, and told the women to get ready to finish it off as soon as it was shot. J. S. took his .410 shotgun, stepped outside with the others following, and shot it. He hit it right in the breast on the first shot. There were only a few other men there and none of them had guns with them, because shells are so hard to get now. As soon as it was hit the others rushed out to club and cut it. My mother had a knife to slash it, and I hit it on the head three times with my club. J. M. and his wife had clubs as did P. C. and his wife. G. S. had an axe and she cut it in half with one stroke. We didn't take turns. We hit at it whenever we got the chance. Then J. S. gathered up the remains, put them in a pile and lit it. We all stood around and watched until it was all burned up, and then went home. E.B.

While individual sorcery presumably will never be practised on this reservation (Lac Court Oreilles) again, as the last shaman died in the winter of 1943, it is probable that public sympathetic magic such as this will continue for some time, although even now it is rarely practised.

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TOTEMIC INSULT AMONG THE WISCONSIN CHIPPEWA

An effective method of teaching and maintaining respect for the totemic animals, birds, and fish of the various Chippewa gentes was through the agency of the Gorging Feast, in which one who had insulted a totem was forced to gorge himself with food and drink provided by the members of the gens whose deity had been insulted.

Among the Chippewa, as with the majority of Indian tribes, the relation between the gens and its patron-being is not reckoned in terms of origin and descent, but rather is that of protection, guardianship, and support. To maintain and encourage this tutelage and support, the behavior pattern of the gens was one of consideration and respect not only toward its own deity, but toward those of the other gentes as well. These patron deities were originally selected from the local fauna such as the bear, marten, sturgeon, pike, loon, and crane, which meant that the people were in constant contact with them, a situation requiring controlled behavior. For the majority a respectful attitude was an accepted tradition, but as a deterrent affecting the iconoclast or ill-advised

humorist, the Gorging Feast was evolved. Disrespectful behavior could be either by word of mouth, i.e., a person making insulting remarks about the deity, or by an overt act, as in one case where a person threw a bullhead he had just caught at a member of the Bullhead Gens. In the first type, the insulting remarks were commonly to the effect that the gens animal was hungry or thirsty. Killing of one's own or any other gens' deity was permissible, but humiliating it was not sanctioned.

In a case of deity insult, the insulter was told to come to a certain place at a specified time, or in instances a runner might be sent with tobacco and instructions (the traditional method of invitation among the Chippewa). The insulted person meanwhile would collect some of his gens mates and prepare the feast, although if the person had enough food on hand he might forego this assistance. The food was usually that of the ordinary diet, but in some cases the preferred food of the gens deity was prepared, as in one case where the members of the insulted Bear Gens prepared a feast of nuts, berries, and maple sugar. In more recent times a bowl of whiskey might be combined with or substituted for the food. The object of insult was frequently kept and placed in the food or drink. For example, the thrower of an acorn at a member of the Bear Gens found the acorn in the bowl of whiskey he was required to drink, while another found the catfish he had insulted dried and placed in his bowl of whiskey. In another case the bullhead which had been insulted by being rubbed in some ashes was cleaned, dried, decorated with colored ribbons, and hung over the food as a reminder.

The insulter would arrive at the appointed time, usually with a knowledge of what was in store for him, and frequently with some of his totem mates to help him. He was seated before the food and told to eat it all. Because of the quantity of food the insulter would gorge himself until "he was ready to burst." If he could not finish it all and had no totem mates to help him he might go home and get a few blankets to give to the members. The insulter in some cases became ill as a result of the ordeal, but the primary punishment was psychological, i.e., the humiliation he would feel at being required to gorge himself at a feast given in honor of the *dŏdem* (totem) he had insulted. Although the insult was commonly done in jest, and much laughing, good-natured taunting, and joking took place during the feast, there was an undercurrent of seriousness, especially on the part of the insulted members. Most of my informants described totemic insult as "a pretty serious affair," while one person thought of it as "a challenge which must be met." He proudly added that his gens had never been known to fail to meet the challenge. No ill-will existed between the members of the insulted gens and the insulter after the "eat-all" feast was over. The effectiveness of the method is suggested by the fact that I could find no instance in which a person once punished in this manner ever repeated his imprudent act.

None of my informants had ever heard of insult by hanging the totemic animal upside down (its orientation on a grave marker) over a sleeping person as reported for the Chippewa of Ontario.[1] Neither had they heard of a person without food deliberately insulting his own totem by hanging the animal head down and putting his lips up to it as if eating it, in order to force his totem mates to give him a feast (also reported for Ontario).[2]

I have selected the following actual cases to illustrate minor variations:

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[1] Landes, R., Ojibway Sociology, pp. 39.

[2] Father Sieber, Personal communication.

My grandmother and a fellow named Whiskey Jim were out fishing and caught a bullhead. They stopped at my Aunt's (her *dodem* was bullhead), and he told her that when they were pulling it up the bullhead kept saying "beer, beer, beer." My Aunt didn't say anything, but the next day she sent her boy down to get a quart of alcohol. Then she invited Whiskey Jim over. Jim took his tobacco pouch, put his pipe in it, and went over. When he got there, my Aunt told him to sit down and fill his pipe with some tobacco she had placed in a dish. He did so, and then she told him that the bullhead must have been calling for this, and she showed him a big dish of alcohol which she set in front of him. She told him to drink it up. He took a big drink and said if he knew then what he knows now he wouldn't have said that about the bullhead. He drank some more of it and finally fell over and went to sleep. He got up about midnight and went home. J. B.

I know one fellow who insulted a member of the Bear Clan by throwing acorns at him. The member got some of his *dodem* mates and they fixed up a big feast of things the bear eats, like berries, nuts, etc., and the fellow and some of his *dodem* mates had to come over and eat until it was all gone. C. C.

One time down at ricing camp we heard a loon cry and then saw it fly right over us. Old John B. said "That fellow must be drunk. He came from Whiskey Lake and now he must be going over to Alcohol Lake to finish up." Some of them who heard him say that belonged to the Loon *dodem*. They got together and sold some wild rice and bought some whiskey and alcohol, and cooked a lot of meat, wild rice, and blueberries. They didn't say anything until it was all ready and then called John B. over. They gave him a dish of whiskey and told him the loon had left it when he came by. He drank it down and they gave him a dish of alcohol. They told him this came from Alcohol Lake. Some of John's *dodem* members who had been invited came along then and they were just going to start eating when John keeled over. The others cleaned up the food and the rest of the whiskey and had a good time. J. M.

Once a fellow who had caught some bullheads took one and threw it, hitting a member of the Bullhead *dodem*. The fellow who had been hit called the other Bullheads together, and they cooked up a lot of wild rice and deer stew. They laid the bullhead near the food, and a runner with tobacco went over and got the other fellow. They sat him down and told him to eat it all, and that they were so proud of their *dodem* they were giving this feast for him. The fellow who had done the insulting had rounded up some of his *dodem* mates to help him, so they all sat down and ate until they were stuffed. J. B.

About seven years ago some Eagle *dodem* members stopped at our house, and one of them said "What's the matter with the martens (our gens). Are they all dry?" They had been drinking and their question meant that they were trying to get more. We told them that the Eagle shouldn't be thirsty because they can fly around and get all they wanted to drink. That question, however, was like a challenge to us, and we think a lot of our *dodem*, so we took it up. Luckily we had quite a bit of whiskey around, so we asked them to come in. We gave them plenty to drink for the next three days. They just stayed with us and drank and slept. Whenever one of them would wake up we would ask him in a sarcastic voice if he thought the martens were still dry, and give him some more to drink. They finally had enough of it and left. It really is a pretty serious affair, but we all had a pretty good time, too. We drank a little too, but our main job was to fill them up. L. M.

While most of the people know to what gens they belong the gens system no longer controls marriage and is functional only in a few minor ways. With the disintegration of this institution the Gorging Feast has lost most of its significance, although one was held as recently as seven years ago.

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RITCHIE TO JOHNSON

In the October-December, 1944, issue of the *American Anthropologist*, there appeared what can only be construed as a basically destructive review by Mr. Frederick Johnson of my recently published dissertation, "The Pre-Iroquoian Occupations of New York State." Since it contains several direct misstatements and insinuations and is thoroughly subjective throughout, I feel obliged to offer this rebuttal.

I should like to make clear at the outset that I must accept the sole responsibility for the methodology employed in the presentation, for the study and interpretation of the material and its classificatory systematization, as well as for the style of writing to which Johnson reacts so violently. This statement is equally true of the actual excavations, except as explicitly stated; hence the writer is fully aware of the limitations of the data, and has referred to this fact where necessary in his candidly tentative treatment of the amassed evidence.

To an unprejudiced reader who will trouble to consider in their proper relations to the text the graphs (derived from the trait tables) given on Plate 165 and summarized in the tables on pages 329–340, it will readily be apparent that the total evidence afforded by every site, down to the last identifiable fragment, has been assessed in the formulation and classification of the culture entities and the associated conceptualizations. The data have been dealt with inductively and the approach has been from the individual component, through recurring complexes, to the larger and more generalized groupings of the classification. There is, thus, no basis for Johnson's repeated allegations that my systematization of the New York cultures depends upon "the manipulation of arbitrarily selected types of artifacts," and consequently as "an empirically imposed scheme it lacks the authority which an hypothesis developed by more strictly inductive methods would carry" (p. 532).

Anent this, Johnson completely repudiates the validity of the concept of diagnostic complexes in studying cultural relationship. Because the plummet and many more elaborate types of the bannerstone have a far wider distribution than the northeast, he would deny their utility as criteria of the Laurentian (or any other) manifestation, in a complex comprising also other particularistic traits (the stone gouge, ground slate points and knives, etc.), despite the recurrence of all or a majority of these elements in the various postulated focal specializations of this aspect, which together cover a wide range in space and perhaps also in time in the northeastern area.

His stricture regarding the "intellectual exercise" whereby I have (always tentatively) suggested placing the "Red Paint culture" of Maine and the Tadoussac and Labrador sites in the Laurentian category of the classification (p. 532) completely ignores the fact that this is as nothing compared with his own amazing feats of mental legerdemain, by means of which he causes the whole Laurentian culture to dissolve into a few generalized types, viz., "notched and stemmed arrowpoints, celts, gouges and adzes" (p. 533), and this in the face both of typological and stratigraphic evidence to the contrary. His reasoning here is of the same utterly unrealistic order as that displayed in his article "Problems Surrounding the Classification of Certain Culture Complexes in New England," (*American Antiquity*, Vol. III, No. 2), where he alleges that because in tabulating approximately 200 deposits of the "Red Paint culture" from eight cemeteries he had found no such thing as a "typical deposit" (i.e., "no one combination

of types which is repeated"), "the custom of depositing artifacts in red ochre may not have become formalized to a degree where certain types of graves containing certain types of artifacts were recognized by the aborigines" (p. 162). Therefore, despite overwhelming typological evidence (and even at that time some stratigraphic evidence as well) to the contrary, there is no such entity as the "Red Paint culture" of Maine, and burials containing red ochre reported from Massachusetts, Vermont and Ontario are introduced to prove it, notwithstanding the palpable fact that these contained a wholly different complex of traits, which I have consequently assigned to a different category (the Middlesex Focus, also disputed by Johnson). "There are none so blind as those who will not see."

One of the curious features of the review is the way in which Johnson rephrases my conclusions and offers them in refutation. A good example of his restatement of my expressed views occurs in the long first paragraph of page 535. Herein also is found the specific allegation, "The continuity, changes, and gradual evolution of culture in the northeast will never be fully brought out by any such system as Ritchie has employed. However, the minor categories which he has developed are essential and they will be a great aid if their limitations are fully recognized. Beyond this we must depend upon an appraisal which is inductive and at the same time flexible enough to allow for frailties of the human intellect" (p. 535). Unfortunately, Johnson has neglected either here or elsewhere, that I can discover, to reveal this ideal method.

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EDITORIAL NOTES

Associate Editor Dr. A. Irving Hallowell has kindly agreed to accept appointment as Review Editor. Books for review and all correspondence regarding reviews should be sent to him at the Department of Anthropology, Northwestern University, Evanston, Illinois.

The attention of contributors is called to the editorial policy of placing bibliographies at the end of each article. They are requested to observe this practice and format in the preparation of contributions.

NOTES AND NEWS

DEMOBILIZATION AWARDS OF THE SOCIAL SCIENCE RESEARCH COUNCIL

In addition to its usual fellowships and grants-in-aid, for which the final application date has already passed, the Social Science Research Council offers this year Demobilization Awards to social scientists of exceptional promise whose careers have been seriously disrupted by war service. They may be for the support of either training or research projects or both. The purpose is to assist the recipients in resuming their professional work with the least delay and maximum efficiency. The details are much more flexible than in the fellowship grants, and made to suit individual requirements. Eligibility is limited to men and women under 36 years of age who are citizens of the United States or Canada, and have either received the doctoral degree or made outstanding records as advanced graduate students in social science. As a general rule the Council seeks recommendations from persons acquainted with individuals of outstanding promise rather than applications from the candidates themselves. For further information address the Secretary to the Committees, Laura Barrett, 230 Park Avenue, New York 17, N. Y.

INTERNATIONAL AFRICAN INSTITUTE

In a recent circular the International African Institute, London, points to "the importance, for scientific research and no less for practical administration, of a systematic survey of the ethnic groupings, distributions and salient social conditions of the peoples of Africa." The Institute, maintaining close liaison with the administrations of various colonial powers, proposes to initiate an "Ethnographic Survey" by co-ordinating the research of the many private and governmental institutions with an aim to achieve this task. "The survey would also seek to establish connections with all field workers currently engaged in Africa, and with technical and administrative officers and missionary organizations able to assist the work." The immediate task would be the evaluation of existing material and its amplification by first hand reports; later existing gaps should be filled by field research.

The Survey, in order to be as useful as possible, should include the following data:

1. Tribal and sub-tribal groupings, indicating variant native, official and other nomenclature.
2. Delimitation of territory, with reference to accompanying map series.
3. Linguistic affiliations of ethnic groups, lingua franca, etc.
4. Demographic data, including size and composition of tribal and other groups, types of settlement, extent of periodic and longer term migrations and tribal mixture.
5. Outstanding features of the physical environment, indicating main physical divisions significant in relation to economic and other cultural sub-divisions.
6. Main features of the economy, including regional variations; participation in external trade, including western enterprises; trends of economic change.
7. Outlines of existing social structure, including both officially recognized institutions of local government and other modern organizations.
8. Agencies of development, operating among the people, e.g. schools, hospitals, services affecting public health, agriculture and stock-raising, marketing organizations, co-operatives, indicating the part played by external official and private agencies.
9. Annotated bibliography classified in relation to the above.

The Institute solicits memberships from persons interested in its work. The annual subscription is £1. Address the Secretary, International Institute of African Languages and Cultures, Seymour House, 17 Waterloo Place, S.W.1, London.

REPORT

PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION FOR THE YEAR ENDING DECEMBER, 1944

The American Anthropological Association held its forty-third annual meeting at the Men's Faculty Club, Columbia University, New York City, on the afternoon of December 16, 1944.

COUNCIL MEETING, DECEMBER 16, 2:30 P.M.

In the absence of the President, Vice-President Roberts presided.

The minutes of the Washington meeting, 1943, were not read but approved as printed in the American Anthropologist, Vol. 46, No. 2, pp. 235–242, April-June, 1944.

REPORT OF THE SECRETARY

The President appointed members of the Nominating and of the Program Committees as follows:

Nominating Committee: Frank M. Setzler, *chairman*, A. V. Kidder, A. L. Kroeber.

Program Committee: C. E. Guthe, *chairman*, H. Hoijer, C. Kluckhohn, W. M. Krogman.

The President appointed W. C. Bennett as alternate for R. H. Lowie to attend the meeting of the American Council of Learned Societies in January, 1944. A. I. Hallowell and R. Linton (J. M. Cooper as alternate) were appointed as delegates to the annual meeting of the American Academy of Political and Social Science.

Upon request of the American Library Association, through the American Council of Learned Societies, for a list of outstanding books in anthropology published in the United States since 1939, the President appointed J. S. Slotkin to draw up the list under the direction of M. J. Herskovits and C. Kluckhohn. The purpose of the list was to guide the American Library Association in the purchase of books to be distributed by them after the war to institutions and scholars in Europe and Asia. The Executive Committee authorized the presentation of an honorarium to J. S. Slotkin upon completion of his part of the work. The approved list was presented to the American Library Association in May, 1944.

Invitations to the AAA to meet jointly with the American Sociological Society and with the American Association for the Advancement of Science had to be declined by the Executive Committee.

In accordance with the Council vote of December, 1943, to leave the selection of the place of the 1944 Annual Meeting in the hands of the Executive Committee, the Committee voted that the meeting be held in New York on December 16, 1944. As instructed at the 1943 Council Meeting, the Secretary presented to the Executive Committee a memorandum summarizing the views expressed at the meeting in regard to the possibility of holding some type of informal program in connection with the 1944 meeting. After deliberation the Executive Committee voted not to sponsor a program meeting, but requested the Secretary to get in touch with the American Ethnological Society to see if that Society would be willing to hold a discussion meeting at the time the AAA would meet in New York.

The Executive Committee voted favorably upon the request of the International Journal of American Linguistics that with the resumption of its publication the following statement appear on the title page: "Under the auspices of the Linguistic Society of America and of the American Anthropological Association."

In June, John R. Swanton retired from the Bureau of American Ethnology and at the same time resigned as our representative to the American Documentation Institute. Henry B. Collins, Jr., was appointed by the President to replace Swanton as AAA representative.

Upon receipt of word that the President would be out of the country for some months, leaving

in September, the Executive Committee, acting in accordance with our constitutional requirement (art. 5, sect. 5), selected Vice-President Roberts to act in the President's place.

In November, Vice-President Roberts appointed N. C. Nelson and G. Reichard to act as observers at the organization meeting of the National Council of Scientific, Professional, Art and White-Collar Organizations which was to have been held in New York on December 2nd. The meeting however was postponed.

Vice-President Roberts appointed the following Committee on Resolutions to report at the Annual Meeting: H. Shapiro, *chairman*, H. J. Spinden, G. Weltfish. He also appointed the Executive Committee to act as Budget Committee.

Vice-President Roberts appointed L. C. Wyman to act as alternate in place of A. V. Kidder as our representative at the meeting of the American Council of Learned Societies to be held in January, 1945.

The report on membership of the Association, as of November 30, 1944, is as follows:

Regular Members—			
AAA members in good standing	686		
AAA members in arrears for 1944	55	741	
AES members in good standing	200		
AES members in arrears for 1944	14	214	
ASW members in good standing	58		
ASW members in arrears for 1944	—	58	
CBS members in good standing	82		
CBS members in arrears for 1944	13	95	
Total membership			1108

There are 10 life members included in the above figures.

The Association has lost through death the following members: George E. Arthur, C. H. Burr, H. U. Hall, Alfred Stephen Kenyon, Philip A. Means (former life member) and George H. Shinn.

In addition to the above the Association was obliged to withdraw a total of 53 names from the membership list: 28 names withdrawn from the AAA for non-payment of dues, and 18 resignations from the AAA; 7 names withdrawn on notification from the AES. The total loss of membership from all causes is, therefore, 59.

During the year 1944, 108 new names were added to our membership list, including 76 new members and 32 new institutions. The membership recorded in 1943 was 1053. The membership recorded on November 30, 1944, was 1108, a net gain of 55 members.

Respectfully submitted,

REGINA FLANNERY, *Secretary*

The Council voted to accept the report of the Secretary.

REPORT OF TREASURER

(As of November 30th, 1944)

The current balances of the three funds of the Association appear on the books as follows:

Operating Fund	$ 8,663.81
Permanent Fund	7,387.61
Memoir Fund	565.06
Total	$16,616.48

Of the above, $13,183.78 is deposited in New York Savings Banks, $100 is in the form of an interest-bearing government bond; and the balance of $3,332.70 is in a checking account at the National City Bank of New York.

The surplus balance in the checking account of the Operating Fund is $2,079.79 (Cash on hand November 30th, 1943—$1,252.91; cash on hand November 30th, 1944—$3,332.70; difference is $2,079.79).

The 1943 Annual Report showed cash on hand in the checking account as of November 30th of $2,252.91. Immediately after the report was approved, $500 was transferred to the Memoir account (Seamen's Saving) and $500 was transferred to the Operating Savings account (Central Savings), which accounts for the difference of $1000 in the balance which we mention above as of November 30th, 1943.

Details of Operating Fund

Balance on hand November 30th, 1943 (Cash plus Savings account)....		$7,008.68	
Less transfer to Memoir Fund.....		500.00	
			$6,508.68
Gross Receipts:			
American Anthropological Association:			
1941.....	$ 6.00		
1942.....	15.00		
1943.....	75.00		
1944.....	3,444.71		
1945.....	682.90		
1946.....	14.85	$4,238.46	
American Ethnological Society:			
1943.....	10.00		
1944.....	988.00	998.00	
Anthropological Society of Washington:			
1944.....	295.00	295.00	
Central States Branch:			
1943.....	141.00		
1944.....	392.00	533.00	
Reimbursements:			
Authors' Reprints.....		269.02	
Reprint Series.....		6.97	
Sales.....		1,011.51	
Interest on Savings.....		75.34	7,427.30
			$13,935.98
Gross Disbursements:			
American Council of Learned Societies.....	$ 35.00	$ 35.00	
Inter-American Society of Anthropology and Geography	3.00	3.00	
American Council on Education (Special authorization by vote of Council).....	10.00	10.00	

J. S. Slotkin for American Library Association project—AAA book recommendations—(Special grant voted by the Executive Committee)	100.00	100.00	
Geo. Banta Publishing Co.:			
Printing	2,444.23		
Illustrating	201.56		
Distribution	200.63		
Reprints	392.99		
Storage/Insurance	80.75	3,320.16	
Editor's Account:			
Traveling Expense	26.25		
Assistant	960.00		
Office Expense	100.17	1,086.42	
Secretary's Account:			
Assistant	100.00		
Office Expense	24.73	124.73	
Treasurer's Account:			
Reimbursements	40.29		
Assistant	480.00		
Office Expense	72.57	592.86	
		5,272.17	
Balance on hand November 30th, 1944		$8,663.81	
			$13,935.98

Details of Permanent Fund

Balance on hand November 30th, 1943		$7,276.35	
Interest on Savings Account		108.01	
Interest on bond		3.25	
Balance on hand November 30th, 1944			$7,387.61

Details of Memoir Fund

Balance on hand November 30th, 1943		$ 616.37	
Transferred from Operating Account	$500.00		
Reimbursement by Mason Mem. #61	3.63		
Interest on Savings Account	8.82	512.45	
		1,128.82	
*AAA cost of Memoir #63	317.87		
**AAA cost of Memoir #64	245.89	563.76	
Balance on hand November 30th, 1944			$565.06

* Additional cost of $75.00 for Memoir #63 paid by American Council of Learned Societies.

** Additional cost of $245.89 for Memoir #64 paid by Viking Fund.

The following is a record of unpaid items due the Association for 1944:

AAA membership	$300.00
CSB membership	65.00
AES membership	70.00
Sales	50.89
Reprint Reimbursements	92.85
Total	$578.74

EXPENDITURES AGAINST BUDGET FOR 1944

	Allowed	*Expended*	*Balance*
Secretary's Expense—			
Assistant	$ 100.00	$ 100.00	$ —
Office Expense	75.00	24.73	50.27
Editor's Expense			
Assistant	960.00	960.00	—
Office Expense	200.00	100.17	99.83
Treasurer's Expense			
Assistant	480.00	480.00	—
Expense	125.00	72.57	52.43
AMERICAN ANTHROPOLOGIST			
Printing and Illustration	3,740.00	2,645.79	1,094.21
Reprints	325.00	392.99	Over 67.99
Distribution	300.00	200.63	99.37
Storage and Insurance	90.00	80.75	9.25
Traveling Expense for Officers	200.00	26.25	173.75
American Council of Learned Societies	35.00	35.00	—
Inter-American Society for Anthropology & Geography	3.00	3.00	—
American Council on Education (Authorized by Council after Budget)	10.00	10.00	—
American Library Association Project (Special grant by Executive Committee after Budget)	100.00	100.00	—
Memoir Fund		563.76	

Respectfully submitted,
BELLA WEITZNER, *Treasurer*

The Council voted to accept the report of the Treasurer subject to the report of the Auditing Committee.

REPORT OF THE AUDITING COMMITTEE

The undersigned, appointed as Auditing Committee, report that they have examined the Treasurer's accounts, as submitted for the fiscal year 1944, and have found them correct.

GORDON EKHOLM, *Chairman*
GLADYS REICHARD
SHERWOOD L. WASHBURN

REPORT OF THE EDITOR

During the year 1944 the American Anthropological Association has published four issues of the AMERICAN ANTHROPOLOGIST (575 pages) and two supplementary publications: Memoir 63, No. 2, Part 1 of Volume 46, *The Sociological Role of the Yoruba Cult Group* by William R.

Bascom (75 pages), and Memoir 64, No. 2, Part 2 of Volume 46, *The Maori: A Study in Acculturation* by H. B. Hawthorn (130 pages).

The Editor herewith tenders his resignation from the editorship and wishes to express his sincere gratitude to the members of the American Anthropological Association and affiliated Societies whose willing and friendly cooperation has made possible whatever success the AMERICAN ANTHROPOLOGIST may have enjoyed under his direction.

Respectfully submitted,
RALPH LINTON, *Editor*

The Council voted to accept the report of the Editor.

REPORTS OF THE COMMITTEES

In the absence of the Chairman and other members of the Nominating Committee, Vice-President Roberts read the proposed slate submitted by the Nominating Committee. Vice-President Roberts withdrew his own name as nominee to represent the Association in the Inter-American Society of Anthropology and Geography on the grounds that he already represented another group with this Society. Gordon Ekholm was nominated from the floor for this office. The Council voted to accept the amended slate and requested that it be presented at the Annual Meeting for election.

After some discussion it was voted that in the future the tentative slate proposed by the Nominating Committee be published in the number of the American Anthropologist issued previous to the Annual Meeting.

The Council voted to accept the applications for membership in the Association of 76 members and 32 libraries and institutions.

W. D. Strong reported that the activities of the Ethnogeographic Board are being continued under the directorship of Henry B. Collins, Jr. In regard to the Inter-American Institute, John Cooper stated that it is at the moment marking time but that three or four of the projects previously started are still under way, including the personality study in Mexico, a health project in Nicaragua, a soil erosion project in Ecuador, and a recording study of Indian music in Mexico and Guatemala. Ralph Linton attended the meeting of the American Academy of Political and Social Science which was held in Philadelphia. It is his opinion that little of the work carried on by the Academy is of interest to anthropology. Our representatives to the Social Science Research Council, the National Research Council, and the American Council of Learned Societies, respectively, stated that they had no reports to make at this time.

The Council voted to leave the determination of place and date of the 1945 Annual Meeting to the Executive Committee. Discussion from the floor stressed the need for a full-scale program meeting. It was voted that the Council urge the Executive Committee to explore possibilities of holding a meeting with papers at the earliest possible moment.

Under date of May 5, 1944, Chairman Shapley of the American Association for an International Office for Education asked the support of the American Anthropological Association for this project. The Council voted that the President appoint a committee to investigate the situation and to report to the Executive Committee, which would then take such action as it sees fit.

The Council voted that the American Anthropological Association join as adherent member the International Institute for Afroamerican Studies.

After hearing the reports and recommendations of N. C. Nelson and G. Reichard, and after discussion from the floor, the Council voted to withhold action regarding membership in the National Council of Scientific, Professional, Art, and White-Collar Organizations for another year, and to maintain contact with it through observers who would be appointed by the President to report at our next Annual Meeting.

The Council voted to renew membership in the American Council on Education for one year.

The Council meeting then adjourned.

ANNUAL MEETING, DECEMBER 16, 4:30 P.M.

Vice-President Roberts presided.

The report on actions taken by the Council, with one exception, was dispensed with in view of the fact that the Annual Meeting followed immediately after the Council Meeting. The Council action regarding the publication of the slate of the Nominating Committee in the American Anthropologist was, however, brought up for further discussion. It was voted that, instead of publication in the Anthropologist, a provisional slate be submitted by notice through the mails to the full membership not later than three weeks previous to the Annual Meeting, and that notification of place and date of meeting be sent at the same time.

The amended report of the Nominating Committee was presented by Vice-President Roberts. After presentation of the nominations the following officers, representatives to councils and associations, and Council members were unanimously elected.

President, Neil M. Judd
Vice-President, W. C. McKern
Vice-President, F. G. Speck
Secretary, Regina Flannery
Treasurer, Bella Weitzner
Editor, J. Alden Mason
Associate Editors: D. S. Davidson, A. I. Hallowell, Ralph Linton, Julian H. Steward
Executive Committee: Robert Redfield, Leslie Spier, Clark Wissler
Representative to Social Science Research Council: W. C. Bennett (1945–47)
Representatives to National Research Council: Douglas S. Byers, W. M. Krogman, Cornelius Osgood (1945–48)
Representatives to Sect. H, American Association for the Advancement of Science: P. H.Nesbitt, Leslie A. White (1945)
Representative to American Council of Learned Societies: John M. Cooper (1948)
Representative to Inter-American Society of Anthropology and Geography: Gordon Ekholm

Council

Members for term to 1948 inclusive: R. L. Beals, W. M. Beckwith, R. F. Benedict, John W. Bennett, M. Butler Lewis, C. S. Coon, C. A. DuBois, George Devereux, M. M. Edel, Paul Fejos, W. N. Fenton, E. F. Greenman, E. Gunther, B. Haile, A. I. Hallowell, C. F. Harding, D. G. Haring, J. S. Harris, E. W. Haury, R. F. Heizer, J. Henry, M. J. Herskovits, G. Herzog, W. W. Howells, A. E. Jenks, N. M. Judd, William H. Kelly, R. Kennedy, F. de Laguna, M. Lantis, R. Linton, J. M. Longyear III, D. Mandelbaum, J. C. McGregor, H. P. Mera, N. Morss, John B. Murra, J. S. Nusbaum, L. M. O'Neale, M. K. Opler, W. Z. Park, A. C. Parker, H. Pollock. F. G. Rainey, G. Reichard, L. Satterthwaite, Jr., C. E. Schaeffer, C. C. Seltzer, L. Sharp, D. B. Shimkin, Bernard Siegel, J. S. Slotkin, A. H. Smith, E. H. Spicer, A. Spoehr, J. H. Steward, O. C. Stewart, M. W. Stirling, R. J. Terry, M. Titiev, R. Underhill, C. Wagley, Richard Waterman, J. Weckler, H. A. Wieschhoff, A. Woodward.

Members for term to 1947 inclusive: W. R. Bascom, W. C. Bennett, L. Bloomfield, P. H. Buck, D. Byers, W. B. Cline, D. S. Davidson, E. W. Gifford, J. P. Gillin, W. D. Hambly, L. Havemeyer, H. Hoijer, M. Jacobs, F. Johnson, E. Kennard, C. R. Keyes, C. Kluckhohn, N. Knowles, L. L. Leh, T. F. McIlwraith, R. McKennan, P. S. Martin, J. A. Mason, M. Mead, A. Metraux, G. P. Murdock, P. H. Nesbitt, O. Ricketson, A. H. Schultz, D. Scott, F. M. Setzler, W. D. Strong, G. C. Vaillant, C. F. Voegelin, H. Webster, L. A. White, H. G. Wittfogel.

Members for term to 1946 inclusive: D. A. Baerreis, K. Bartlett, J. Bram, P. A. Brannon, I. C. Brown, J. L. Champe, K. M. Chapman, H. S. Colton, L. S. Cressman, T. Deuel, J. Dollard, K. P. Emory, G. Engerrand, J. C. Ewers, R. Flannery, A. H. Gayton, H. S. Gladwin, J. B. Griffin, C. E. Guthe, A. Halpern, A. Hansen, C. L. Hay, W. W. Hill, L. W. Jenkins, P. Ka-

berry, I. T. Kelly, O. LaFarge, W. LaBarre, A. Lesser, O. Lewis, T. M. N. Lewis, J. Lips, E. Loeb, E. H. Morris, J. A. Noon, M. E. Opler, C. Osgood, B. D. Paul, H. Powdermaker, V. F. Ray, W. A. Ritchie, F. H. H. Roberts, Jr., I. Rouse, H. C. Shetrone, J. B. Stetson, R. J. Sullivan, S. Tax, W. W. Taylor, Jr., H. Tschopik, H. Turney-High, W. L. Warner, G. Weltfish, B. Weitzner, G. D. Williams, G. R. Willey, F. R. Wulsin.

Members for term to 1945 inclusive: J. Adair, B. Aginsky, W. Andrews, C. M. Arensberg, M. F. Ashley-Montagu, H. G. Barnett, G. Bateson, J. Birdsell, E. G. Burrows, A. Caso, E. D. Chapple, D. Collier, H. B. Collins, Jr., B. Cummings, A. C. L. Donohugh, F. H. Douglas, P. Drucker, F. Eggan, L. C. Eiseley, G. Ekholm, J. F. Embree, C. S. Ford, J. A. Ford, G. Foster, Jr., H. Getty, I. Goldman, G. Gorer, M. Haas, E. T. Hall, Jr., O. S. Halseth, J. R. Hanks, J. C. Harrington, G. G. Heye, E. A. Hooton, A. E. Hudson, J. Jennings, N. Joffe, V. H. Jones, F. Keesing, D. Keur, A. Kidder, II, S. T. Kimball, V. Kinietz, H. W. Krieger, W. M. Krogman, W. Lessa, W. Lipkind, S. K. Lothrop, R. C. MacKaye, J. G. McAllister, T. D. McCown, G. McGregor, W. C. McKern, H. S. Mekeel, H. Miner, M. Mook, R. G. Morgan, H. L. Movius, Jr., P. Nash, D. Oliver, R. L. Olson, P. Phillips, R. Redfield, E. K. Reed, P. Reiter, H. L. Shapiro, M. Siegel, L. W. Simmons, E. Siskin, V. J. Smith, M. W. Smith, F. G. Speck, D. Spencer, L. Spier, W. S. Stallings, Jr., S. Stubbs, G. L. Trager, C. Wagley, W. D. Wallis, L. Ward, H. N. Wardle, W. R. Wedel, A. Whiting, J. W. M. Whiting, R. Zingg.

Past Presidents (*ipso facto* members of the Council): F.-C. Cole, J. M. Cooper, F. W. Hodge, D. Jenness, A. L. Kroeber, R. H. Lowie, G. G. MacCurdy, N. C. Nelson, R. Redfield, L. Spier, H. J. Spinden, J. R. Swanton, A. M. Tozzer, C. Wissler.

COMMITTEE RECOMMENDATIONS

The Executive Committee acting as the Budget Committee presented the following recommendations for the 1945 budget, which was unanimously adopted:

1. Secretary's Office		
Secretary's assistant	$ 100.00	
Office expenses	75.00	$ 175.00
2. Editor's Expenses		
Editor's assistant	960.00	
Office expenses	200.00	1,160.00
3. Treasurer's Expenses		
Treasurer's assistant	480.00	
Office expenses	125.00	605.00
4. American Anthropologist		
Printing and Illustration	3,000.00	
Reprints	350.00	
Distribution	250.00	
Storage and Insurance	90.00	3,690.00
5. American Council of Learned Societies		35.00
6. Inter-American Society for Anthropology and Geography		3.00
7. American Council on Education		10.00
8. Traveling expenses for secretary, editor, and treasurer to annual meeting (also to include any possible expenses of meeting, December 16, 1944)		200.00
9. Removal of Editorial Office from New York		50.00
Total		$5,928.00

It is also recommended that $1,500.00 be transferred from the balance of the Operating Fund ($8,663.81) as follows:

To the Memoir Fund	$ 500.00
To the Permanent Fund	$1,000.00

The Committee on Resolutions (Spinden acting as chairman in absence of Shapiro and Weltfish) presented the following resolutions which were adopted:

1. *Be it resolved:* That the American Anthropological Association express its sincere thanks to Ralph Linton for his highly meritorious services as Editor of the AMERICAN ANTHROPOLOGIST for six years.

2. *Be it resolved:* That the American Anthropological Association record its deep sense of loss in the death of Philip Ainsworth Means who joined problems of colonial history with those of prehistoric anthropology in Latin America.

3. *Be it resolved:* That the American Anthropological Association record its deep sense of loss in the deaths of the following members: George E. Arthur, graduate student at the University of Chicago, who entered the Naval Reserve in 1942 and who was killed in action; C. H. Burr, Professor of Mental Diseases at the University of Pennsylvania; H. U. Hall, investigator in Siberia and Sierra Leone for the University of Pennsylvania Museum, Philadelphia; Alfred Stephen Kenyon; George H. Shinn.

4. *Be it resolved:* That the American Anthropological Association express its sincere thanks to the American Ethnological Society for its kind hospitality.

5. *Be it resolved:* That the American Anthropological Association express the hope that in the settlement of this present World War the rights of so-called primitive peoples to way of life and the possession of property be respected, and that these peoples be not enslaved under any political device whatsoever. We express the hope that principles of ethics be given preference over doctrines of sovereignty in the future intercourse of nations to this end: that selfish war be eliminated and just peace maintained.

REGINA FLANNERY, *Secretary*

The following Committees have been appointed for 1945:

Nominating Committee: John R. Swanton, *Chairman*, Fay-Cooper Cole, H. L. Shapiro.

Program Committee: Frank H. H. Roberts, Jr., *Chairman*, J. O. Brew, Cornelius Osgood, W. Duncan Strong, George C. Vaillant.

NEIL M. JUDD, *President*

ANTHROPOLOGICAL SOCIETY OF WASHINGTON, REPORT

The Anthropological Society of Washington at its annual meeting held January 16, 1945, elected the following officers: *President*, T. Dale Stewart; *Vice-President*, Regina Flannery; *Secretary*, William N. Fenton; *Treasurer*, Waldo R. Wedel; *Members of the Board of Managers*, W. M. Cobb, Wm. H. Gilbert, Alfred Métraux, Maurice A. Mook, Julian H. Steward.

A report of the membership and activities of the Society since the last annual meeting follows:

Membership:	
Life members	1
Active members	63
Associate members	24
Total	88

This represents an increase of 21 over last year.

The members elected during the year were, Active Members: Dr. John M. Armstrong, Rev. Hugh Bihler, S. J., Mr. Huntington Cairns, Mr. Joseph Blake Eggen, Mr. Wm. Leo Hansberry, Mrs. Anne Fromme Hertford, Dr. Frederick S. Hulse, Dr. Edwin M. Loeb, Mr. Ernest Maes, Dr. Peveril Meigs, Mr. John A. Pope, Sister Lucia Van der Eerden, Dr. A. J. Waring, Jr.; Associate Members: Mrs. Susan W. Armstrong, Miss Elizabeth Bacon, Lt. Frederica de Laguna USNR, Mr. Gordon W. Hewes, Lt. W. W. Howells USNR, Lt. Comdr. A. H. Leighton USNR, Dr. Dorothea C. Leighton, Dr. Margaret Mead, Dr. J. S. Slotkin, Dr. Laura Thompson, Dr. George L. Trager, Capt. Wm. L. Van Ness.

We have lost through death one member: Dr. William H. Spinks, active member since 1942.

The Anthropological Society of Washington records its deep sense of loss at the death of this member, and desires to extend its sincere sympathy to his wife.

The report of the Treasurer follows:

Funds invested in Perpetual Building Association (With interest to July 1, 1944)..	$1,789.35
21 shares Washington Sanitary Improvement Co. (Par value $10 per share).....	210.00
2 shares Washington Sanitary Housing Co. (Par value $100 per share).....	200.00
U. S. Savings Bond, Series G.....	500.00
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The Society acted as host to the Society for American Archaeology on the occasion of its annual meeting May 13, 1944, at the Cosmos Club, Washington, D.C.

All regular meetings were held at the U. S. National Museum. The May meeting was the occasion of a dinner in honor of Dr. Ethel John Lindgren at the National Zoological Park Restaurant. Continuing a practice adopted in 1942 of keeping the mailing list current with the roster of anthropologists in Washington, an average of 180 notices has been sent out for regular meetings. The Washington Urban League members were invited to the November meeting, and the social scientists in the Department of Agriculture were invited to sit with us in January. Six hundred invitations went out for Mrs. Roosevelt's lecture, including the membership in the Washington Academy of Sciences.

The Society has continued to enjoy the presence in the Capital of a large number of anthropologists. Some of them have joined our ranks and others have delivered papers. The programs of meetings during the first months of the year continued under the direction of a committee headed by the Secretary. This group, having served two years, requested that a new committee be appointed. The President appointed Dr. Julian H. Steward, *chairman*, and asked Dr. Gordon R. Willey and Dr. Waldo R. Wedel to serve with him. Turning from the anthropology of war areas, with occasional discussions of theoretical anthropology, the new committee initiated a series of symposia: first, on New World Prehistory, followed by a series of discussions of Anthropology and Contemporary Problems.

Titles of papers presented before the regular meetings of the Society were as follows:

January 18, 1944, 717th meeting, Dr. Gordon T. Bowles, "Probable Developments in the Field of Comparative Racial Anatomy."

February 15, 1944, 718th meeting, Eleanor R. Roosevelt, "Civilization in the South Pacific."

March 21, 1944, 719th meeting, Ruth Evans Pardee, "Some Aspects of Minankábau Society."

April 18, 1944, 720th meeting, Dr. Homer G. Barnett, "Emotional Factors in Cultural Change."

May 31, 1944, 721st meeting, Dr. Ethel John Lindgren, "Some Current Trends in British Anthropology."

October 17, 1944, 722nd meeting, Dr. Gordon R. Willey, Dr. Frank H. H. Roberts, Jr., and Dr. Waldo R. Wedel, "Symposium on Dating in New World Prehistory."

November 21, 1944, 723rd meeting, Discussions of Anthropology and Contemporary Problems—I, Dr. Raymond Kennedy, "Anthropology and Colonial Administration."

December 19, 1944, 724th meeting, Discussions of Anthropology and Contemporary Problems—II, Symposium on Race Contacts and Race Conflicts: Dr. Morris E. Opler, "American White—Oriental Relations"; Dr. Arthur Raper, "American White—Negro Relations"; and Dr. Alfred Metraux, "Race Relations in Latin America."

WILLIAM N. FENTON, *Secretary*

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